*The* JOURNAL
OF THE
FRANKLIN INSTITUTE
OF THE STATE OF PENNSYLVANIA
*DEVOTED TO SCIENCE AND THE MECHANIC ARTS*

VOL. CLXXIII MARCH, 1912 No. 3

# THE NATURE OF SOLUTION.[1]

BY

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[The work herein discussed has been carried out with the aid of grants from the Carnegie Institution of Washington.]

WHEN you asked me to speak on solution I felt that you had asked me to speak on the most important condition of matter known to man. This statement is a strong one and may lead you to suspect that you are going to hear something sensational. If so, I fear you will be disappointed.

What is meant by speaking of solution in the terms that I have used, is that it is matter in this condition which gives us *nature*, and, consequently, *natural science*.

I am, of course, using the term solution in the *broader sense*. When the word solution is first used we are liable to think of such systems as are produced when cane sugar or common salt is thrown into water, and to limit our conceptions to systems of this type. In a word, we are liable to think of solutions in liquids as solvents, or, even more narrowly, of solutions of solids in liquids.

A solution is a mixture of matter in any state of aggregation,

[1] Presented at the meeting of the Section of Physics and Chemistry held December 14, 1911.

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with matter in the same or in a different state of aggregation, fulfilling the condition that the constituents cannot be separated mechanically. If we mix two gases that do not act chemically upon one another, we have a true solution of a gas in a gas. In a similar manner we have solutions of liquids and solids in gases. A liquid or a solid throws vapor into the superincumbent gas—dissolves in it. It may be objected that the liquid or solid first passes into vapor and this then mixes with the gas. It can be replied that when cane sugar (a solid) dissolves in water (a liquid) it does not remain in the solid state; the dissolved substance acquires the state of aggregation of the solvent.

Similarly, we have solutions of the three states of matter in solids as the solvents, and one of these types of solutions, that of solids in solids, is interesting and important. There belong in this class the isomorphous mixtures, the alloys, etc.

The question arises, What justification have we for calling these mixtures of solids, solutions? The answer is, they have the three fundamental properties of true solutions—lowering of the freezing point of the solvent, lowering of the vapor tension of the solvent, and osmotic pressure.

There are, then, nine types of solutions; matter in every state of aggregation dissolving in matter in the same and in every other state of aggregation.

Using the term solution even in this broad sense, the question still remains, Why designate it as the most important condition of matter? What justification is there for such a statement? Can anyone think of a chemical reaction in which solution of some kind is not involved? I do not mean by a chemical reaction simply a change of phase or state of aggregation of matter, such as the melting of ice or the boiling of water, but a transformation in which new products are formed. The number of such reactions, if they exist at all, is such a small percentage of the total number of chemical reactions that it is practically negligible. The whole science of chemistry, then, is a branch of the science of solutions. And what justification is there for calling chemistry a branch of the science of solutions? Do solutions concern other branches of natural science than chemistry?

Geology deals with the sedimentary rocks deposited from aqueous solutions, and with the igneous rocks which came out of molten magmas or solutions at elevated temperatures. Much

of geology is, therefore, a branch of the science of solutions. And take the biological sciences. Think how many of them owe their very existence to solutions! What would physiology, pharmacology, or therapeutics be without solutions?

Taking all of these facts into account—and there are many more which will occur to anyone who thinks about this problem—you will see that there is some justification for the stress that I am inclined to lay upon the importance of matter dissolved in matter for natural science as a whole. The chemist, the geologist, the biologist, and also the physicist must be interested in the problem of the nature of solution.

What the real condition of matter in the dissolved state is, was practically unknown until it was taken up by that really great man of science, Van't Hoff, whose untimely death we have recently been called upon to lament. Certain ideas had been expressed as to what was the nature of dissolved substances, but these were hardly more than unsubstantiated guesses, and for the most part erroneous.

While it may be impossible to define a genius, we know that Van't Hoff was one, and of the first water. From very meagre and imperfect data he was able to foresee relations that have placed the whole subject of solutions upon a scientific basis.

From a few measurements of the osmotic pressure of a very few substances at a very few dilutions, Van't Hoff was able to show that we can deal with solutions fundamentally as we deal with gases—the laws of gas-pressure apply to the osmotic pressure of solutions. But why was the discovery of this relation so important? Because we know incomparably more about matter in the gaseous state than in the liquid or solid condition. We might almost say that the fundamental problems in connection with gases have, for the most part, been solved. We know comparatively little about matter in the liquid state, and our ignorance of the fundamental problems in connection with solids is almost perfect. To be sure, we know something about the passage of heat, light, electricity, etc., through solids, and the geometrical forms in which they crystallize, but, after all, these are comparatively superficial matters.

Now that we can apply the laws of gas-pressure to solutions, and since we can deal with gases by the only exact method known to man—the mathematical—as in thermodynamics, it

follows that we can apply this method, in part at least, to solutions, and deal with them by the truly scientific method.

We have seen how important solutions are not only for chemistry, but for geology, the biological sciences, and, indeed, natural science in general. In being able to deal with solutions by the exact method, we have made a beginning towards introducing this method into the hitherto purely empirical branches of science. Indeed, in the case of chemistry very much progress has been made in this direction, so that to-day chemistry is well advanced towards becoming an exact branch of natural science.

The laws of gases, then, apply to the osmotic pressure of solutions. This is the truth, but very far from the whole truth. The laws of gas-pressure apply only to the osmotic pressure of dilute solutions of those substances whose aqueous solutions do not conduct the current, to dilute solutions of nonelectrolytes. But the nonelectrolytes comprise only about one-half of the chemical compounds, and by far the least interesting half. All of the acids, all of the bases, and all of the salts belong in the class of electrolytes, and these are the substances that make chemistry what it is. To the osmotic pressure of not a single one of these compounds do the gas laws apply. They all exert osmotic pressures that are too great in terms of the laws of gas-pressure; therefore, greater than exerted by nonelectrolytes of the same concentration. Van't Hoff recognized this fact clearly, and pointed it out with emphasis in his epoch-making paper [2] in the first volume of the journal that was to be the official organ of the new branch of science which this paper contributed largely to create. Indeed, he formulated the facts for both classes of substances, nonelectrolytes and electrolytes. The gas equation—

$$Pv=RT$$

applied to the osmotic pressures of solutions of nonelectrolytes, while it was necessary to insert a coefficient greater than unity into the equation when it was to be used for electrolytes—

$$Pv=iRT$$

the coefficient in question being the well known Van't Hoff "*i*."

Here Van't Hoff left the problem, and it remained for a

[2] *Zeitschr. phys. Chem.*, 1, 481 (1887).

Swedish physicist, who visited your town and lectured here last spring—Arrhenius—to furnish the explanation of why it is that "*i*" is greater than unity for all electrolytes.

To say that the law of Boyle applies to the osmotic pressure of solutions is the same as to say that osmotic pressure is a property that depends only upon numbers[3]—upon the relation between the number of parts of the dissolved substance and of the solvent—just as gas-pressure depends only on the number of parts in a given volume. If we have too great an osmotic pressure exerted by the electrolytes, it must then mean that we have a too great number of parts of the dissolved substance present in the solution, too great in terms of the molecular concentration of the solution.

We all know the solution of the problem that was furnished by Arrhenius. He broke down the molecules of acids, bases, and salts in solution into parts charged electrically, and these are the *ions*.

The molecule of a simple acid like hydrochloric broke down into two parts—a positively charged hydrogen ion and a negatively charged chlorine ion. The molecule of a simple base like potassium hydroxide broke down into two charged parts—the potassium being charged positively and the hydroxyl negatively. Similarly, a salt like potassium chloride broke down into positively charged potassium and negatively charged chlorine.

In terms of this theory, then, dilute solutions of these types of substances should exert just twice as much osmotic pressure as solutions of the same molecular concentration of nonelectrolytes, and such is the fact.

In a solution of substances like sulphuric acid, barium hydroxide, or barium chloride each molecule breaks down into three charged parts; and dilute solutions of these substances should exert three times the osmotic pressure of equi-molecular concentrations of nonelectrolytes, and here again such is the fact; and it is perfectly simple to extend the theory to any class of electrolytes.

Arrhenius not only showed that this theory, the theory of electrolytic dissociation, could explain the facts of osmotic pressure; but furnished a large number of independent lines of evi-

[3] An arithmetical property of this kind may be designated as a fundamental property, and the most worthy of study.

dence for its correctness, in the very paper[4] in which the theory was first proposed; published it should be noted also in the first volume of the *Zeitschrift für physikalische Chemie;* and this paper was also destined to contribute largely to the science of which this journal was to become the chief exponent.

Here Arrhenius left the subject of solution. He pointed out methods of measuring the magnitude of the dissociation, and showed that high dilution was necessary for complete dissociation. The ions in the solution enjoyed an existence independent of one another, and also largely of the solvent.

There is nothing in Arrhenius's paper to indicate that the ions even in the presence of water were not "dry." At least they were not "wet." There was no evidence at that time that there is any combination between the molecules or ions of dissolved substances and the solvent (say, water). The contribution of Arrhenius to the subject of solution was second in importance only to that of Van't Hoff, and, while absolutely necessary in explaining the phenomena presented by dissolved substances, was far from sufficient. The laws worked out by Van't Hoff, as well as the generalization reached by Arrhenius, applied only to *very dilute* solutions; indeed, to solutions so dilute that they are seldom used in the scientific laboratory.

The objection was raised, and rightly so, that this is all very well for "ideal" or infinitely dilute solutions, but what we want is a theory that will account for the phenomena as presented by solutions in general, and especially by solutions of those concentrations which give us, on the one hand, chemistry, and, on the other, geology and biology. The theory of solutions as given us by Van't Hoff and Arrhenius was a theory of what we might term homœopathic solutions.

It is thus obvious that the problem of the nature of solutions was, then, not finally solved even by Van't Hoff and Arrhenius, but much was left to be done. I believe that some progress has been made in the desired direction in recent times, and I shall aim to give you a glimpse of how this has been done.

About a dozen years ago a Japanese by the name of Ota came to my laboratory by an unusual route, which touched at the bottom of the Pacific Ocean. The boat on which he came to this country foundered outside of the Golden Gate, and he was

[4] *Zeitschr. phys. Chem.*, 1, 631 (1887).

one of the few who came to the surface and was rescued. It would be expected that such a man would do something extraordinary after he arrived, and he did.

He was set to study the lowering of the freezing point of water produced by concentrated solutions of electrolytes. It is well known that dissolved substances, in general, lower the freezing point of the solvent in which they are dissolved. Sea water freezes lower than pure water. This property of solutions, like osmotic pressure, is also an arithmetical property; that is to say, the magnitude of the freezing-point lowering of the solvent depends only on the ratio between the number of parts of the solvent and of the dissolved substance. Knowing the dissociation of any solution, we can easily calculate what its freezing point should be.

The work of Ota brought out the fact that the lowering of the freezing point of water by concentrated solutions of electrolytes was greater, very much greater, and in some cases many times greater, than would be calculated from their concentration and their dissociation.

The lowering of the freezing point of water being a function of numbers, and numbers only, the molecular lowering—*i.e.*, the actual lowering divided by the concentration expressed decimally—would increase with the dilution of the solution, since the percentage dissociation increases with the dilution. Ota found that for fairly concentrated solutions not only did the molecular lowering of the freezing point not increase with the dilution, but increased with the concentration, and in some cases to several times its calculated value.

This, for the time being, placed the whole subject of freezing point lowering in about the worst possible position. Everything found was directly at variance with what would be expected from any theory known at that time. What were we going to do with these facts? At first we doubted them, and then repeated the work. The same results were obtained a second and a third time, and we had to accept them as corresponding to a truth of nature.

Then how were we going to explain them?

A possible explanation, and the only explanation I could think of for these facts, was this:—if a part of the water present which was supposed to be acting as solvent was really there not

as solvent water but in combination with the dissolved substance, the freezing point of the water present, really acting as solvent, would be lowered more than if there was no water combined with the dissolved substance—the freezing point of a part of the water present would be lowered more by a given amount of dissolved substance than if all the water present was acting as solvent.

While this was a possible explanation, I did not believe it was the true one when I proposed it in 1900.[5] The work of my laboratory for the next few years was devoted almost exclusively to obtaining evidence bearing upon this very point. If in solution a part of the water is present as water of hydration, then there ought to be some method or methods of proving it.

The following line of work suggested itself. Measure with reasonable accuracy the freezing point lowerings produced by a large number of substances dissolved in water, at different concentrations; and see how general abnormally great freezing point lowerings are, and what is the order of their magnitude as presented by the different types of electrolytes. It was soon found that practically all electrolytes give abnormally great lowerings of the freezing point of water, but the magnitude of these lowerings referred to molecular quantities—the magnitude of the molecular lowerings—differed greatly from one type of compound to another. In the case of such substances as potassium, sodium and ammonium chlorides, bromides, iodides, and nitrates the observed lowerings of the freezing point of water produced by them are only slightly greater than the lowerings calculated from the concentrations of the solutions in question and the dissociation.

Substances of the type of calcium, strontium, magnesium, etc., chlorides and nitrates, lower the freezing point of water two or three times as much as would be calculated; while compounds such as the chloride and nitrate of iron, aluminum and chromium in concentration solutions, give four or five times as much lowering of the freezing point of water as would be expected from their concentration and dissociation.

We studied over a hundred typical compounds, and demonstrated the general character of the phenomenon in question as

[5] *Amer. Chem. Journ.*, 23, 103 (1900).

far as salts are concerned. This line of thought then occurred to us: If the explanation that we have offered to account for these abnormally great freezing-point lowerings is true—*i.e.*, combination between the ions or molecules of the dissolved substance and the water—then, other things being equal, those substances that combine with the largest amounts of water in solution should be the ones that would bring the largest amounts of water with them out of solution. In a word, there should be a *relation between the magnitude of hydration in solution and water of crystallization.*

An examination of the data then in hand for some twelve hundred solutions, showed that the relation in question undoubtedly existed. Take all of the chlorides with which we worked and plot the curve diagram.[6] Let the ordinates be molecular lowerings of the freezing point, and the abscissæ concentrations of the solutions; we find those substances with no water of crystallization near the bottom of the diagram, which means that they produce relatively little lowering of the freezing point of water.

Going up the diagram we find next those substances which crystallize with two molecules of water, and then high up on the diagram those that crystallize with six molecules of water of crystallization. The relation between water of crystallization and freezing point lowering is perfect so far as the chlorides are concerned. Take the nitrates—the corresponding curve diagram for these substances is even more interesting than for the chlorides, because a larger number of nitrates than chlorides were studied; the nitrates in general being more soluble than the chlorides.

At the bottom of the diagram we have the nitrates of sodium, potassium, and ammonium which have no water of crystallization. A little higher up the nitrate of lithium with two molecules of water of crystallization. Then comes the nitrate of calcium with four molecules of water of crystallization. Then a half dozen curves closely bunched, corresponding to six nitrates each crystallizing with six molecules of water of crystallization; and,

[6] For the diagrams in question see: *Amer. Chem. Journ.*, 33, 536 and 539 (1905); *Zeitschr. phys. Chem.*, 49, 433 and 436 (1904); Carnegie Institution of Washington, Publication 60, 21 and 24 (1907).

finally, well up towards the top of the diagram the nitrates of aluminum, iron and chromium, the first having eight and the second and third nine molecules of water of crystallization.

In a similar manner curve diagrams were plotted for the bromides studied and for the iodides investigated, with the same general result that was brought out for the chlorides and for the nitrates.

The relation, then, between lowering of freezing point and water of crystallization is perfect so far as the chlorides, bromides, iodides, and nitrates are concerned; and a fairly large number of each class of compounds was studied; and this seems to be a necessary consequence of the theory of hydration in aqueous solution.

Take just one other line of evidence furnished by water of crystallization. We know that chemical aggregates or complexes in general are less stable the higher the temperature. Therefore, hydrates would be less stable the higher the temperature to which the solution was subjected. As this is true of hydrates in solution, it would be true of the hydrates which dissolved substances can bring with them out of solution. In a word, substances should crystallize with less and less water the higher the temperature at which they crystallize. We knew in a general way that this was the fact; but not until we examined the literature on the subject did we realize how many examples illustrating this point there are on record.

Just one or two examples, selected from many,[7] will be given. (See p. 227.)

These data suffice to show that water of crystallization is a function of temperature, which, again, is a necessary consequence of the hydrate theory.

All in all,[8] there have been more than a dozen independent lines of evidence discovered, which point unmistakably to the correctness of the view that when salts are dissolved in water

[7] *Amer. Chem. Journ.*, 34, 294 (1905).

[8] *Amer. Chem. Journ.*, 31, 303 (904); 32, 308 (1904); 33, 534 (1905); 34, 291 (1905); 35, 445 (1906); 38, 683 (1907); 39, 313 (1908); 40, 355 (1909); 41, 19 (1909). *Zeitschr. phys. Chem.*, 46, 244 (1903); 52, 231 (1905); 55, 385 (1906); 74, 325 (1910). Publications of the Carnegie Institution of Washington, Nos. 60, 80, 110, 130, and 160.

more or less of the water is combined with the salt. It would lead us too far here to go into these in any detail.

| | |
|---|---|
| $CaCl_2$ ..... $H_2O$ | |
| $CaCl_2$ ..... $2H_2O$ | |
| $CaCl_2$ ..... $4H_2O$ | As the temperature at which the crystals are formed is lower and lower. |
| $CaCl_2$ ..... $6H_2O$ | |
| $MgCl_2$ ..... $6H_2O$ | Elevated temperatures above 20°. |
| $MgCl_2$ ..... $8H_2O$ | |
| $MgCl_2$ ..... $10H_2O$ | 20°. |
| $MgCl_2$ ..... $12H_2O$ | — 10° to — 12°. |
| $MnCl_2$ ..... $2H_2O$ | 20°. |
| $MnCl_2$ ..... $4H_2O$ | 15°. |
| $MnCl_2$ ..... $6H_2O$ | — 21°. |
| $MnCl_2$ ..... $11H_2O$ | — 21° to — 37°. |
| $MnCl_2$ ..... $12H_2O$ | — 48°. |

Having obtained such an abundance of independent evidence that there is hydration in aqueous solution, and having calculated the approximate composition of the hydrates, the next question that arose was, Is water the only solvent that combines with substances dissolved in it? Water is the most general solvent, and has greater power to break down molecules of electrolytes into ions than any other common solvent. All things considered, water is by far the most remarkable chemical compound known to man, but does it monopolize the power of combining with dissolved substances? In a word, is there any evidence that solvents other than water combine with substances dissolved in them?

Take, for example, the alcohols—methyl and ethyl alcohols. They are very good solvents. Do they combine to any extent with substances in solution in them?

It is obviously impossible to throw any light on this question by the freezing-point method. Alcohol does not freeze at a temperature which can be measured with sufficient accuracy for the present purpose. Its freezing temperature is too far removed from ordinary temperatures to permit of it being measured to within a few thousandths of a degree, and such measurements, to have any value in connection with the problem in hand, must be of this order of accuracy.

Fortunately, we have another method that can be used in such connections—*i.e.*, the method based upon the property of a

salt to lower the vapor-tension or raise the boiling point of the solvent.

If we study the rise in the boiling point of a solvent produced by a dissolved substance, we soon learn that rise in boiling point obeys the same laws as lowering of freezing point. Just as we calculate the freezing point of any solution from its concentration and dissociation, just so we can, from the same data, calculate its boiling point, and, consequently, the rise in the boiling point of the solvent produced by the dissolved substance.

We calculated the boiling points of solutions of a fairly large number of substances, and then determined the boiling points of these same solutions experimentally.[9]

Results were obtained by the boiling point method which were of the *same general character* as those obtained by the freezing point method. The values found experimentally were larger, and in many cases much larger than the calculated values. Just as we interpreted too great freezing point lowering of water by dissolved substances as being due to hydration, and obtained so many independent lines of evidence in favor of the correctness of this interpretation, just so we interpret the abnormally great rise in the boiling point of a solvent by a dissolved substance as being due to a combination of the solvent in question with the substance dissolved in it. In the case of solutions in alcohol we have *alcoholation;* in acetone, *acetonation;* if the solvent is glycerol, *glycerolation,* etc.

In the case of solvents in general we have *solvation,* and our conception, instead of being limited to water as a solvent where we have hydration, becomes for solutions in general *the theory of solvation.*

We have already referred to the large number of lines of evidence in favor of hydration in aqueous solution. We shall see a little later that there is just as good evidence for combination between solvent and dissolved substance, when alcohol, or acetone, or glycerol is used as the solvent, as when water is employed.

I shall take up only one other of the many lines of evidence that we brought to bear upon the solvate theory of solution.

Solutions of certain substances are colored, while solutions of certain other substances are colorless. Why is this? This

[9] *Amer. Chem. Journ.,* 32, 338 (1904); 35, 136 (1906).

raises the question as to what is meant by transparency or opacity to light. Glass is transparent because the visible wave lengths of ether vibrations find nothing in the glass which they can throw into resonance with themselves. The metals, even in thin films, are opaque because the vibrations in the ether called light find something in the metal which they can throw into resonance with themselves. They do so, setting up vibrations within the metal. The energy of these ether vibrations is expended in setting up vibrations within the metal; the ether vibrations cease, and since it is these vibrations falling upon the retina that produce the sensation which we call light, we say the light is absorbed by the metal, or that the metal is opaque.

Similarly, when white light falls upon a colored solution, some of the wave-lengths find something in the solution which they can throw into resonance with themselves. These wave-lengths are cut out and the remaining wave-lengths pass on through. The color of the solution in question is determined by the wave-lengths that pass through it.

We have been studying for about seven years, with the aid of grants very generously awarded by the Carnegie Institution of Washington, the light that passes through solutions of colored salts. In a word, we have been studying *the absorption spectra of solutions.*

The work consisted in allowing light to pass through the solution, then fall on a concave grating, and then upon a photographic plate. The wave-lengths that were cut out by the solutions would appear as dark lines or bands upon the photographic plates. Something more than six thousand solutions have already been studied from this stand-point.

The question that arises here is, What bearing could such work have on the question of solvation in solution? A moment's thought will make this clear. Given a dissolved particle without asking whether it is molecule, atom, or electron; it will show resonance for certain wave-lengths of light. Now load that particle down by allowing it to combine with a number of molecules of the solvent. In this condition it is highly probable that it would show different resonance than when not combined with the solvent. Further, the amount of the solvent with which the dissolved substance was combined would also affect its resonance; and we can readily change this amount by simply changing the concentration of the solution, the temperature,

adding a dehydrating agent, etc. Only one of the many problems that have been studied here by the absorption spectra method will be discussed.

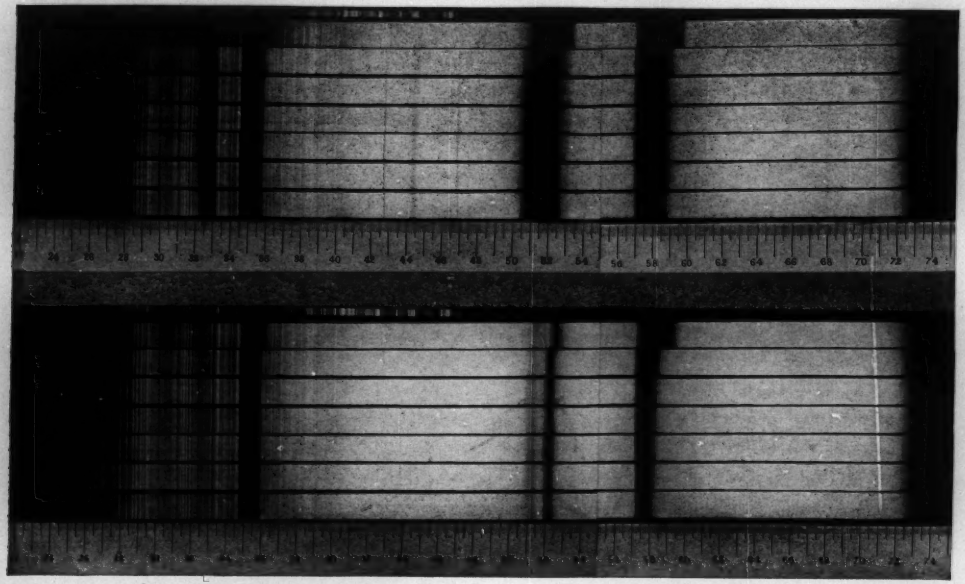
The resonance of a dissolved particle will be affected by the solvent combined with that particle. Suppose, then, that we dissolve a given colored salt in different solvents, we would have different solvates in the different solvents, and we would expect these different solvates of the same salt to show different resonance with respect to light. For illustration and to fix the idea, neodymium chloride dissolved in water would form a hydrate, in methyl alcohol a methyl alcoholate. We would expect the hydrated salt to show different light resonance from the alcoholated salt.

On examining the literature on this point before our own work was begun, we found the above conclusion negated on every hand. Indeed, we found nothing but the statement that the absorption spectrum of a compound is absolutely independent of the nature of the solvent in which the compound was dissolved, provided, of course, that the solvent itself had no absorption spectrum.

The number and unambiguous character of the lines of evidence that we had already discovered, bearing upon the problem of solvation in solution, had convinced us once for all as to the general correctness of the solvate theory. As there is combination between the solvent and the dissolved substance, this should manifest itself in the light resonance of the compound. In a word, we could not accept what was recorded in the literature, and at first Dr. Uhler, and then Dr. Anderson and myself, therefore, took up this line of work.

The results that were obtained for neodymium coloride in methyl alcohol and in mixtures of methyl alcohol and water are shown in Plate 1. The uppermost strip of *A* corresponds to a solution of neodymium chloride in pure methyl alcohol. The second strip corresponds to a solution of neodymium chloride of the same concentration in methyl alcohol containing 16⅔ per cent. of water. It will be seen that the absorption is very different in the pure alcohol from what it is in the mixed solvents. Succeeding strips correspond to the addition of more and more water. Strip 3 from the top corresponds to 33⅓ per cent. water, strip 4 to 50 per cent. water, strip 5 to 66⅔ per cent. water, strip 6 to 83⅓ per cent. water and strip 7 to pure water.

PLATE I.



A

B

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PLATE II.

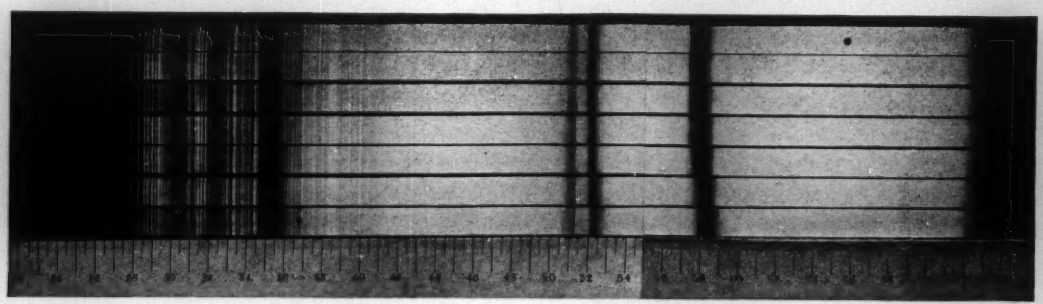

PLATE III.

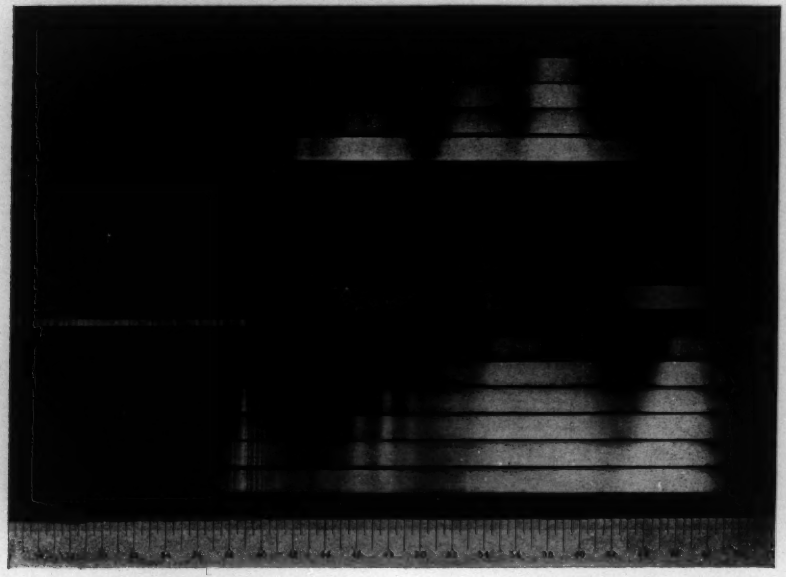

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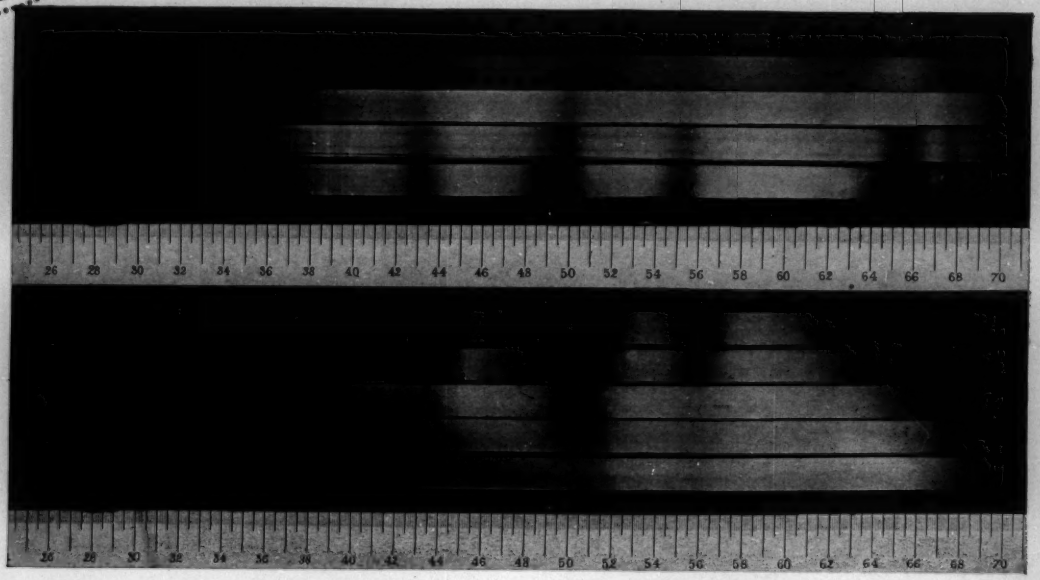
PLATE IV.



It will be seen that the entire change in the absorption spectrum takes place on the addition of 16⅔ per cent. of water to the methyl alcohol, and that the absorption in water is quite different from in alcohol.

In order to determine whether there was simply a shift in the position of the alcohol bands on the addition of water, or whether there were really new absorption bands in the water, *B*, Plate I, was taken. In this case a much more dilute solution of neodymium chloride was used. In the uppermost strip we have again the salt dissolved in the pure alcohol. In the second strip we have the salt in a mixture of methyl alcohol and 16⅔ per cent. of water, and in the succeeding strips more and more water is present. A comparison of the uppermost strip with strip 2 will show that in the two cases we are dealing with very different absorption bands. The number and structure of the bands in the pure alcohol are very different from the bands when there is water present. In a word, the "water bands" are not the "alcohol bands" simply shifted in position, but *entirely different absorption bands.*

We have here, then, a well-defined "alcohol" spectrum and a well-defined "water" spectrum of neodymium chloride, just as we predicted from the solvate theory should exist.

The addition of more than 16⅔ per cent. of water to the alcohol does not further change the absorption spectrum in the case of the more dilute, as in the case of the more concentrated solution of neodymium chloride.

So far as is known to me, this is the first case ever found of the effect of a non-absorbing solvent on the absorption spectrum of a compound dissolved in it—of the existence of well-defined "solvent bands."

Having found that all of the change in the absorption spectrum was produced on adding 16⅔ per cent. of water, we determined to study this change in detail by adding very small and increasing amounts of water to the alcohol.

Plate II represents the results obtained when neodymium chloride was dissolved in a mixture of ethyl alcohol and water. The concentration of the neodymium chloride was constant = 0.5 normal. The percentages of water, beginning with the solution whose spectrum is adjacent to the numbered scale, were 0, 5.5, 10.6, 16, 21.3, 26.6, and 32. The depth of the solution throughout was 0.5 cm.

In the second strip the bands characteristic of the alcohol solution are much more prominent than those belonging to the aqueous solution, while in the third strip the reverse is true. This shows that the composition of the mixed solvent which gives the bands with about half their normal intensity is 7 to 8 per cent. water. There we have both the "alcohol" and the "water" bands coexisting, and they are seen to have very different structures—are very different bands. The "water" bands decrease in intensity as the amount of water present decreases.

Take next solutions in mixtures of methyl alcohol and water. The results are shown in Plate VI, the concentration of the neodymium chloride being 0.25 normal.

The strip next to the numbered scale corresponds to the pure alcohol. As we go away from the scale the strips correspond to more and more water. In strip 3, counting from the bottom, which corresponds to 5.3 per cent. of water, we have both the "water" and the "alcohol" bands of equal intensity. As we go up the spectrogram, corresponding to the presence of more and more water, the "water" bands become stronger and stronger and the alcohol bands weaker and quickly disappear.

We have, then, in mixtures of water and methyl alcohol, as in mixtures of water and ethyl alcohol, perfectly definite absorption bands of neodymium chloride corresponding to each of the solvents present. To see whether "solvent bands" were in any wise limited to neodymium salts, we used salts of other metals in this same connection. Plates III, IV, and V were obtained in the phase of the work carried out by Dr. W. W. Strong and myself.

Plate III is the spectrogram obtained with uranous chloride in acetone, in methyl alcohol, and in glycerol. *A* is the absorption spectrum of uranous chloride in acetone, the different strips corresponding to different depths of solution. *B* is the absorption spectrum of uranous chloride in methyl alcohol, the different strips corresponding to different depths of solution. *C* is the absorption of uranous chloride in glycerol. This spectrogram shows both the uranous and uranyl bands. A glance at this plate will show how different the spectra are in the different solvents.

Plate IV, *A*, gives the absorption spectra of uranous chloride in water to which ethyl alcohol is added, and *B* the absorption spectrum of uranous acetate in water with different depths of

PLATE V.

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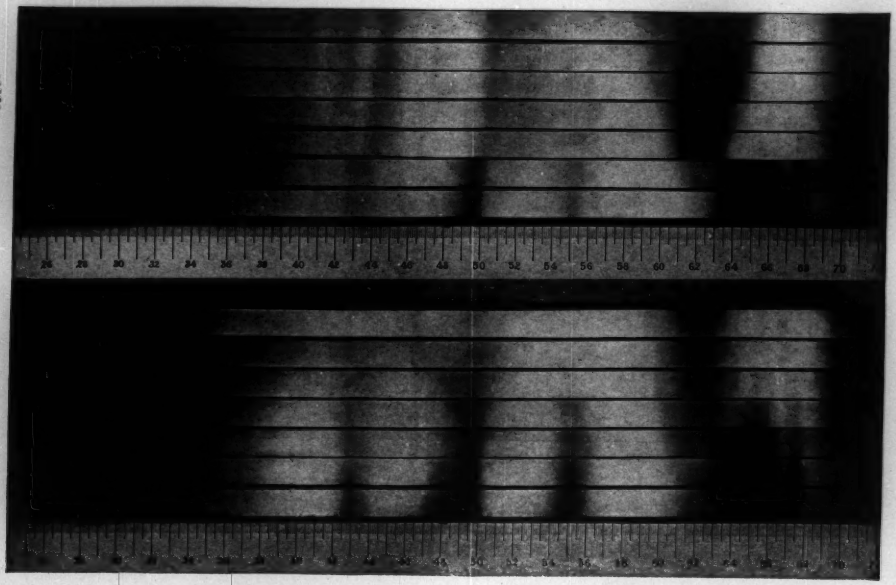
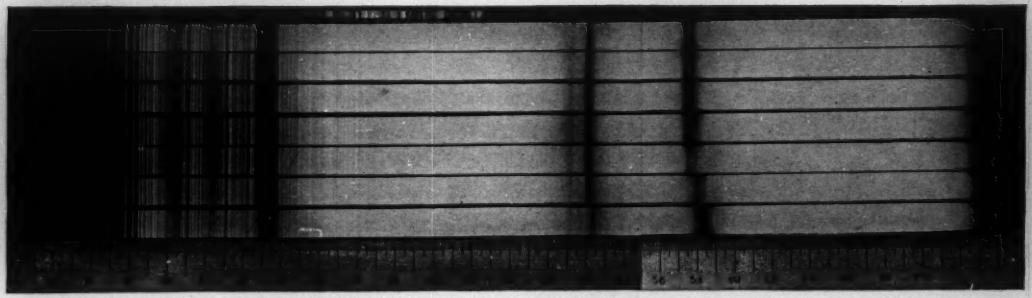


A

B

PLATE VI.



solution. This spectrogram is especially interesting on account of the facts brought out in *A*. The first strip at the bottom of the spectrogram represents the absorption spectrum of uranous chloride in water. The second strip represents the absorption of uranous chloride in a mixture of water and about 25 per cent. of ethyl alcohol. The next strip corresponds to about 40 per cent. of alcohol, and here the water bands are very feeble and the alcohol bands begin to show feebly. In the next strip there are about 60 per cent. of alcohol. Here the "water" bands have practically disappeared, and the "alcohol" bands begin to show out very strongly. In the last strip there is about one-third water and two-thirds alcohol. The "water" bands are now gone entirely, and the "alcohol" bands are alone.

One of the best examples of "solvent" bands is Plate V. *A* is the spectrogram of uranous bromide in a mixture of water and methyl alcohol, and *B* is the spectrogram of uranous chloride in a mixture of water and methyl alcohol.

The aqueous solutions are at the bottom of the spectrograms, and as we go up more and more alcohol is added from strip to strip. We see the "water" bands strong at first, rapidly disappear, and the "alcohol" bands at first weak, increase in intensity as the amount of alcohol present relative to the water increases.

We have here *two entirely distinct sets of bands*, one corresponding to each of the solvents present. A better example than this of solvent bands could hardly be imagined.

Having found such abundant evidence of the effect of the solvent on the power of the dissolved substance to absorb light, in the case of the few solvents referred to above, it seemed very desirable to bring other solvents within the scope of this work, and this has been done. Since the neodymium absorption lines and bands are very sharp, it was selected as the principal substance to be used in this connection. This phase of the work has been carried out recently by himself and Dr. Strong. The following nomenclature is used for the neodymium absorption spectra:

$\alpha$ group in the region $\lambda$ 3400 to $\lambda$ 3600
$\beta$ group at about $\lambda$ 4300
$\gamma$ group from $\lambda$ 4600 to $\lambda$ 4800
$\delta$ group from $\lambda$ 5000 to $\lambda$ 5400
$\epsilon$ group in the region $\lambda$ 5800
$\xi$ group at $\lambda$ 6300

The results for neodymium chloride are:

| | | α GROUP | | | | | |
|---|---|---|---|---|---|---|---|
| | | Weak | Narrow strong | Narrow strong | Narrow strong | | |
| In water | λλ | 3390 | 3465 | 3505 | 3540<br>3560 | | |
| | | Faint-est | | Widest and most intense | | | |
| In methyl and ethyl alcohols | λλ | 3475 | 3505 | 3560 | | | |
| | | Sharp | | | Weak | Strong | Strong narrow |
| In propyl alcohol | λλ | 3445 | 3460 | 3490 | 3510 | 3525 | 3540 |
| | | | | Weak | | | |
| | | 3560 | | 3580 | | | |
| | | All these bands hazy, the middle being the weakest of the three | | | | | |
| In propyl alcohol | λλ | 3460 | 3510 | 3535 | | | |
| | | Sharp narrow | Weak | | | | |
| In butyl alcohol | λλ | 3450 | 3460 | 3492 | | | |
| | | Sharp narrow | | | | | |
| | | 3535 | 3545 | 3560 | | | |
| | | Weak | Strong-er | Very weak | | | |
| In isobutyl alcohol | λλ | 3455 | 3485 | 3515 | 3545 | 3570 | |
| | | Weak | Strong sharp | Strong sharp | | | |
| In glycerol | λλ | 3520 | 3475 | 3550 | | | |

| | | β GROUP | | | | |
|---|---|---|---|---|---|---|
| | | Very sharp | | Narrow weak | | |
| In water | λλ | 4271 | | 4290 | | |
| In methyl and ethyl alcohols | λλ | 4290 | | 4325 | | |
| | | Very weak | | Wide weak | Wide weak | |
| In propyl alcohol | λ | 4270 | 4285 | 4330 | 4450 | |
| | | This is the only band of the β group that shows. | | | | |
| In isopropyl alcohol | λλ | 4265 | | | | |
| | | Are all weak and of the same intensity. | | | | |
| In butyl alcohol | λλ | 4265 | 4285 | and | 4300 | |
| | | Very weak | | | | |
| In isobutyl alcohol | λ | 4300 | | | | |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | | Sharp | | Very fine | | Very fine | |
| In glycerol | λλ | 4288 | | 4270 | and | 4305 | |
| | | γ GROUP | | | | | |
| | | Hazy edges | Very weak | | | | |
| In water | λλ | 4610 | 4655 | 4685 | | | |
| | | Sharp | Narrow intense | | | | |
| | | 4755 | 4820 | | | | |
| In methyl and ethyl alcohol | λλ | 4700 | 4780 | and | 4825 | | |
| | | Weak diffuse | | | | | |
| In propyl alcohol | λλ | 4600 | 4700 | 4770 | and | 4830 | |
| | | Very diffuse | | | | | |
| In isopropyl alcohol | λλ | 4600 | 4690 | and | 4730 | | |
| In butyl alcohol | | No bands appear with wave-lengths between λ 4600 and λ 4800 in the spectrogram taken. | | | | | |
| | | | | Very weak | | | |
| In isobutyl alcohol | λλ | 4700 | 4730 | 4780 | 4830 | 4880 | |
| In glycerol | λλ | 4620 | 4710 | 4730 | 4760 | 4790 | 4840 |
| | | δ GROUP | | | | | |
| | | Narrow | Wide hazy | Intense narrow | | | |
| In water | λλ | 5090 | 5125 | 5205 | 5227 | | |
| | | Narrow | Faint hazy | | | | |
| | | 5255 | 5315 | | | | |
| | | Hazy | Hazy fainter | Intense narrow | | | |
| In methyl and ethyl alcohols | λλ | 5125 | 5180 | 5220 | | | |
| | | Intense | Narrow | Faint | | | |
| | | 5245 | 5290 | 5315 | | | |
| | | | | Wide and diffuse | | | |
| In propyl alcohol | λλ | 5130 | 5180 | 5220 | 5230 | | |
| | | | | Very weak | | | |
| | | 5250 | 5290 | 5330 | | | |
| | | Wide and diffuse | | | | | |
| In isopropyl alcohol | λλ | 5100 | 5320 | | | | |
| | | Narrow | Narrow weak | | | | |
| In butyl alcohol | λλ | 5085 | 5095 | 5130 | 5200 | | |
| | | 5215 | 5240 | 5270 | 5300 | | |
| In isobutyl alcohol | λλ | 5150 | 5260 | 5215 | | | |
| | | 5230 | 5250 | 5300 | | | |
| | | Wide | Narrow | Narrow | | | |
| In glycerol | λλ | 5120 | 5170 | 5190 | Weak | | |
| | λλ | 5230 | 5240 | 5250 | 5270 | | |

| | | ε GROUP | | | | |
|---|---|---|---|---|---|---|
| | | Narrow strong | Strong doublet | | | |
| In water | λλ | 5725 | 5745 | 5765 | 5795 | |
| In methyl and ethyl alcohols | λλ | Hazy edges 5725 5860 Hazy | 5765 Nar'w'r faint 5895 | Strong 5800 Faint 5925 | Very intense 5835 5850 | |
| In propyl alcohol | λλ | 5740 | 5780 | 5810 | | |
| In isopropyl alcohol | λλ | Broad diffuse 5720 | 5780 | 5810 | | |
| In butyl alcohol | λλ | 5750 | 5780 5900 | 5820 5930 | 5860 | |
| In isobutyl alcohol | λλ | Weak 5740 5920 | Strong 5810 5950 Very weak | Strong 5850 5995 Very weak | 5890 6020 Very weak | |
| In glycerol | λλ | Hazy 5740 | 5790 | 5805 | 5820 | 5850 |

The above results obtained with neodymium chloride will show the effect of the solvent on the absorption spectra of this compound. The bands in the different solvents have different wave-lengths and different relative intensities.

Having found that the solvent played such an important rôle in the absorption spectra of the dissolved substance, Jones and Strong used isomeric organic solvents to see whether such closely related solvents would affect differently the absorption of light. They prepared solutions of neodymium chloride in propyl and isopropyl alcohols, and in butyl and isobutyl alcohols, and photographed the absorption spectra of this salt in these isomeric solvents. If we compare carefully the spectra of neodymium chloride in butyl and isobutyl alcohols, we find that the bands are weak and diffuse in isobutyl alcohols, and have different relative intensities from what they have in butyl alcohol. The bands in butyl alcohol are very much finer and sharper than they are in isobutyl alcohol. Further, the bands of neodymium chloride in isobutyl alcohol have slightly greater wave-lengths than in butyl alcohol. To eliminate the possibility of the effect of the solvent on absorption spectra as being due to anything inherent in the nature of neodymium chloride, the nitrate of

neodymium was studied in the same way as the chloride. The absorption spectra of neodymium nitrate in water, in methyl alcohol, in ethyl alcohol, in mixtures of these alcohols with water, in propyl and isopropyl alcohols, in butyl and isobutyl alcohols, in acetone and water, in ethyl ester and in formamide, were carefully photographed and studied. The results will be given, in the case of neodymium nitrate, only for the alpha bands.

NEODYMIUM NITRATE—$\alpha$ BANDS.

In water, practically the same as the bands of neodymium chloride, but the bands of the nitrate are broader and heavier than those of the chloride.
In methol and ethyl alcohols there were only two bands in the $\alpha$ group.

| | |
|---|---|
| | $\lambda\lambda$ 3465 and 3545 |
| In propyl alcohol | $\lambda\lambda$ 3455, 3500 and 3585 |
| In isopropyl alcohol | $\lambda\lambda$ 3460, 3505 and 3535 |
| In butyl alcohol | $\lambda\lambda$ 3450, 3500 and 3540 |
| In isobutyl alcohol | The absorption was so great that in the spectrograph taken the $\alpha$ group did not appear. The absorption in general is the same as that of the chloride in this alcohol. |
| In acetone | $\lambda\lambda$ 3475 and 3555 |
| In ethyl ester | $\lambda\lambda$ 3455, 3500 and 3540 |

The above results suffice to show the difference in the lengths of the absorption bands of the $\alpha$ group of neodymium nitrate in the different solvents. Similar differences manifest themselves when we compare the bands of the $\beta$ or $\gamma$ group for this compound in the different solvents, and it is, therefore, not necessary to make further comparisons here.

Having obtained such marked changes in the absorption bands of neodymium chloride and nitrate, as the nature of the solvent was changed, Jones and Strong extended this work to salts of uranium. A few of the bands of uranyl chloride in the different solvents will be given for the sake of comparison.

URANYL CHLORIDE.

| | |
|---|---|
| In water | $\lambda\lambda$ 4025, 4170, 4315, 4460, 4560, 4740 and 4920 |
| In methyl alcohol | $\lambda\lambda$ 4090, 4220, 4345, 4465, 4590, 4760 and 4930 |
| In ethyl alcohol | $\lambda\lambda$ 4100, 4250, 4400, 4580, 4750 and 4900 |
| In propyl alcohol | $\lambda\lambda$ 4100, 4230, 4400, 4580, 4750 and 4910 |
| In isopropyl alcohol | $\lambda\lambda$ 4100, 4250, 4360, 4560 and 4750 |
| In butyl alcohol | $\lambda\lambda$ 4100, 4240, 4390, 4560, 4750 and 4970 |
| In isobutyl alcohol | $\lambda\lambda$ ..... 4400, 4560, 4720 and 4900 |
| In ether | $\lambda\lambda$ 4040, 4160, 4300, 4444 and 4630 |
| In methyl ester | $\lambda\lambda$ 4030, 4160, 4280, 4440, 4620, 4790 and 4920 |
| In glycerol | $\lambda\lambda$ 4025, 4140, 4260, 4400, 4540, 4720 and 5050 |
| In formamide | $\lambda\lambda$ ..... 4450, 4650 and 4840 |

The above evidence leaves little or nothing to be desired in favor of the view that "solvent" bands are very general in the absorption spectra of solutions; and it will be recalled that this was predicted from the solvate theory of solution at a time when all of the experimental evidence then on record was against this view.

I regard the above line of evidence in favor of the view that there is combination between the solvent and the dissolved substance as little short of proof—indeed, better evidence for the correctness of any theory could scarcely be hoped for. The question still remains, Of what use is the solvate theory of solution, even if it is true?

We have seen that the theory of solution as left by Van't Hoff and Arrhenius is a theory of very dilute or "ideal" solutions. It does not apply to a single solution of the concentration such as we use in the laboratory. While necessary, it was, therefore, far from sufficient.

If we supplement the theory of Van't Hoff and Arrhenius by the solvate theory, we have a theory of solution which applies not simply to the very dilute or "ideal" solution, but to the solutions with which we have to deal in the scientific laboratory—to the solutions which give us, on the one hand, chemistry, and, on the other, geology and biology.

A combination of the theory of electrolytic dissociation with the theory of solvation enables us to deal with the whole subject of solution in a reasonably satisfactory manner; and we have already had some glimpse at the importance of the condition of matter in solution not only for chemistry, but for practically every other branch of natural science. It is obvious that we are dealing here with the most fundamental condition of matter for natural science in general, and any light on the nature of this condition makes for progress in our study of nature.

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**Resistance Alloy of Copper—Manganese—Nickel.** W. B. DRIVER. (*U. S. Patent* 981, 542.)—These alloys contain large percentages of nickel and manganese; the nickel renders the copper-manganese alloys electrically constant. These alloys are non-corrodible and can be machined. An alloy of 60 parts copper, 10 manganese, and 30 nickel has a specific resistance of 65; one of 40 copper, 40 manganese, and 20 nickel has a resistance of 150, as compared with copper = 1.

# BRUSHES.[1]

BY

**W. R. WHITNEY,**

Director Research Laboratory, General Electric Company.

THE object of this paper is to describe some experiments which are being carried out with the view of assisting in an improvement in the qualities of motor and generator brushes, and particularly of the carbon type. It is well known that, while there have been very many improvements in all sorts of electrical apparatus during the past twenty years, there has not been a corresponding improvement in the quality of brushes, at least in this country. It is a peculiar commercial or manufacturing condition—which all engineers will recognize when their attention is called to it—that an electrical manufacturing company usually puts upon its motors and generators all the legitimate accessories of its own make which are possible, excepting the brushes, and forces the users to purchase brushes from companies not necessarily in very close touch with electrical requirements.

If the brush were as simple an article of manufacture as the lag-screw with which the apparatus is attached to the floor, this would not be serious; but quite a different condition exists. In fact, I think it is safe to say that a poor carbon brush or brush of a wrong type, may render inoperative any kind of electrical apparatus. When one considers the stoppage of a long line of electric cars or the temporary shut-down of a generating plant, because of defects in a brush, one wonders that the demands have not already made an art of brush making. One finds, in fact, that the production of this very essential, unobtrusive little block of carbon has hardly had any study at all.

Every electrical machine is carefully designed in detail as to size and shape of copper, iron, and insulation, all of which vary with each machine, but as yet the important characteristics of a brush are not included in the design, although some one grade

[1]Presented at the joint meeting of the Institute and Philadelphia Section, American Institute of Electrical Engineers, Wednesday, January 17, 1912.

of brush is expected to operate satisfactorily over a great range of design. So long as this unfortunate condition exists the best that can be hoped for is a brush which will do fairly well for a large number of machines, very well on a few, and cause continuous trouble on a small balance, until investigation of the constants of the particular machine, or its operating condition, shows the need of a different type of brush.

The careful investigation now in progress leads us to hope that the various characteristics of carbon brushes will soon be sufficiently understood, so that before long the designers of dynamos and motors will be enabled to use a brush exactly fitted to a particular design of machine, rather than a brush which does fairly well on machines of that general size and character.

Apparently the carbon brush was first made from such stock as is used for arc lamp electrodes and dry cell carbons. It was mixed, molded, and baked in practically the same way, and the prices fixed on that basis. We have experimented with the understanding that a product might be producible which could bear a tenfold greater cost of production than ordinary carbon brushes, and it is probable that our present methods are not far from this condition.

In what we call the early days, the motor or generator brush consisted of a brush of leaves of copper, and this had to be treated with the best of care. It was frequently cleaned and oiled, and on some types of machines gave much trouble. It was usually set at an angle, so that the ends of the laminæ bore upon the commutator. Any reversal of direction of rotation, as has frequently to take place in such apparatus as railway motors, was out of the question with this kind of brush. A block of solid copper bearing directly upon the commutator on such machines causes arcking and spattering of the metal, as there is a relatively high current passing from one commutator segment to the adjacent segment through that part of the brush which connects them. This was a reason for laminæ in the brush.

Our first experiments were taken up along the line of previous methods of manufacture, and attempts were made to get at some sort of life tests and standards of comparison. We tried to learn what a good brush had to do and how long it might be expected to do it. In lamp manufacture the testing is an art by itself. An incandescent lamp is made to burn a certain time at a prefixed

efficiency. We questioned whether a carbon motor brush could be submitted to similar life and quality tests. We recognized the importance of learning, if possible, what physical properties or constants for brushes would help in determining quality for electrical uses. The art is undeveloped, so that there is no considerable agreement between engineers even on important points. For example, it would be made clear by one engineer that a brush must be much softer than copper, so as not to wear away the commutator, and must not contain any hard spots, because these might take up metal from the commutator. Against this was the equally powerful argument that the brush must be harder, so as to cut mica and prevent the insulation finally protruding above the copper, and for this reason carborundum, the hardest practical material, has been experimentally introduced into brush mixtures. That the contract resistance should be relatively high and the body resistance low were also practical suggestions. In order to operate systematically, we attempted to choose properties or tests to which numerical values could be attached. It was not assumed that all, or even any, of these factors could be interpreted strictly in terms of brush quality, but some such system of coordinating and comparing experimental products seemed necessary. We would naturally welcome suggestions leading to better tests. There were finally adopted measurements of hardness, tensile strength, density, electrical resistivity, and such mechanical tests as a chip and fracture tests.

The hardness is determined by the Shore Scleroscope, which consists essentially of a diamond-pointed weight which falls freely in a perpendicular glass tube and, striking upon the brush, rebounds to a height in the tube, which height is read from a scale. In general, the harder the material, the higher will be this rebound. In the case of brushes it distinguishes qualities over a range of about seventy units and is of considerable value in determining regularity of product.

A brush as made for railway motors and containing coke, graphite, lampblack, and binder carbon, if properly baked and fired, would have a hardness about 50, while if the firing be interrupted at, say, 500° C., its hardness value would be about 20. If the coke and lampblack be omitted, while the binder carbon is three per cent., and natural graphite be used, the hardness value will fall to about 16, even after firing for maximum hardness.

In other words, such brushes as find common use on electrical apparatus may vary in hardness, depending upon the desired use, from 15 to 75 units of hardness.

The tensile strength is determined by cutting from the brush, held between steel guide plates, a definite testing piece of the carbon which can be held in the grip of the strength-testing machine and which will break at a point where the cross-section is exactly one-quarter square inch. The break is produced by the addition of shot at a definite rate to a pan supported by the test sample. The strength is expressed in pounds per square inch section. The density compared to water is calculated from the weight of the brush and its physical dimensions.

The resistivity is determined by measuring the voltage drop across contact points 1½ inches apart, which points bear heavily upon the brush when a current is sent through the brush. Separate contacts are used for the current. The accuracy of the measurement is about 3 per cent. of itself.

Some sort of chipping test seemed desirable because brushes in use often deteriorate and wear away, due to flaking off of chips from the edges of the bearing surface. This would be expected from the nature of the impacts given a brush when it chatters on a rough armature revolving at high peripheral speed. The chip test we have used consists in a clamp for holding the brush firmly on a solid base or anvil, and in a weight guided by perpendicular rods, which weight falls from a fixed height and strikes the bearing face of the brush repeatedly at the same point, about a millimetre from the edge. The number of impacts necessary to force off a chip is called a chip test. This is quite surprisingly uniform for uniformly made brushes. It is usually weighted so that a brush of best quality will regularly chip after about ten impacts of the weight when dropping from an increasing height in steps of one centimetre each.

Additional qualitative tests have resulted from comparison of product under different conditions. For example, the fracture or appearance of the fresh surface produced by breaking the brush across its longer dimensions discloses very effectively any irregularities produced by improper baking or pressing. This cross-section should be quite homogeneous and the fracture regularly conchoidal or square. No shelves, cracks, or angular markings will be disclosed on breaking a well-made brush. It is

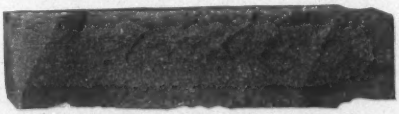
believed that such internal irregularities represent weakened structure, that even miniature cracks might take up and carry copper from the commutator, and that breaking of a brush in use may often be attributed to internal cracks produced in the manufacture. Undoubtedly a rough commutator or heavy pressure of the spring of the brush holder accounts for the breakage of many brushes in use, but in our tests on operating machines these variables are kept under control as much as possible.

FIG. 1.

Inferior brush fracture.

To illustrate this point, Figs. 1 and 2 are introduced. Fig. 1 shows a typical case of an improperly baked brush. The ingredients are the same as in Fig. 2, which was pressed and baked with greater care. The imperfections noted in 1 are produced by too rapid heating. They are not discoverable in the brush before breaking it.

FIG. 2.



Good brush fracture.

Neither of these is a squirted brush (*i.e.*, cut from a bar squirted through a die), both having been pressed to the desired shape from the powdered mixture.

The composition of the mixture from which the brush is molded has been varied greatly to suit various requirements, but in general it is made up of two or more of the four elements: lampblack, finely-ground petroleum coke, graphite, and some kind of tar or petroleum pitch which serves as a binder.

The effect produced by each of these ingredients is a different one, and a suitable balance seems necessary and varies with the

use of the brush. A brush made mostly of lampblack, with suitable binder, would be dense and hard, but of poor conductivity, and would cut copper badly. One made mostly of graphite is usually too soft and on ungrooved commutators wears away too rapidly. The mica between the bars gradually protrudes and wears away the brush. Its electrical resistance is very low. One made mostly of coke is hard and of high resistance. It usually cuts the copper badly. The petroleum coke is used because of its uniformity and freedom from mineral matter. It can readily be appreciated that for some special purposes it may be well to incorporate hard polishing material into a brush, as where much mica has to be cut and where high conductivity, and therefore much graphite, is desired. The accidental grains of sand or such mineral impurities as often occur in ordinary coke, on the other hand, are to be carefully excluded.

It might seem that by using the above four forms of carbon (for the pitch leaves a fourth form of carbon, after the heat treatment) a satisfactory product could be obtained, and it has become apparent that the qualities may be varied over a wide range by proper variation; but the requirements are continually advancing, so that all sorts of experiments in composition, such as adding sulphur, special oils, etc., have been carried out.

It may be stated as a general rule that, except for special conditions, the finer the state of subdivision of the brush ingredients, the better the quality of the finished brush. We have found, after having used ball mills, that the Raymond impact grinder pulverizes coke and similar materials very perfectly. We have found it desirable to grind not only the coke but also the mixture of all the ingredients, to insure thorough mixing and a dense brush.

The finely-divided materials, coke, lampblack, and graphite, are placed in a mechanical dough mixer and a solution of the pitch in benzol is added and the whole kneaded for several hours. The benzol is then driven off by heat and the dried product, which is then quite hard, is reground to about 200 mesh, and this powder is compressed into brush form, in steel molds.

It was first thought that the best way to form the brush was to squirt bars of the mixture by means of a hydraulic press. These bars could then be cut to desired length. So far as our work went along this line, the product was not as satisfactory

as when the brushes were individually pressed. Experiments were made in which various proportions of pitch binder were used. The temperatures of the press, mold, and brush material were carefully controlled; special presses were built, etc., but irregularities often appeared in the product. The tendency of the material to flow unequally within itself during the passage through the die seemed to be responsible for cleavage planes and internal curved surfaces, which would often not develop until the brush was completed by baking, and even then could only be disclosed by breaking the brush. We do not mean to conclude that a satisfactory brush cannot be made by squirting the hot mixture through a die, as the reverse is probably true, but for our purposes we finally adopted the accurate weighing of the mixture for each individual brush and the compression of this weighed material to an exact size in a mold under such a high pressure that additional pressure produces no further change. This pressure, for a standard railway motor brush, is about 25,000 pounds per square inch. Above this pressure the quality is not appreciably affected; much below it, inferior product, as shown by density, resistance and strength, results. Marked effect is produced by variation in the temperature at which the mixture is pressed. For this reason care is taken to have the pressing done at about 25° C.

The pressed brushes are then packed in cast-iron boxes, in lots of 100 to 200, and covered with a liberal layer of fine coke. A cast-iron cover which well fits the box is then inserted, and this, in turn, is covered with coke dust. A pile of these boxes is shown in Fig. 3. This box fits snugly into the electrically heated muffle shown in Fig. 4.

Early in the investigation it became evident that a large part of the irregularity of product, the shrinkage-cracks, etc., were to be attributed to a too rapid rate of rise of temperature during the early stages of heating the pressed brush. The pitch, or binder, which on its fractional distillation leaves the cementing carbon, which in turn largely determines the hardness and strength of the brush, is a mixture of organic compounds which liquefies at moderate temperature (100° C.) and rapidly evolves gases far below red heat. This led us to test methods of electric heating, and the rate of distillation of the volatile materials at gradually-rising temperature was determined. This work resulted

finally in the adoption of a very small type of muffle, for commercial production, which had a capacity for not over 200 brushes. It was heated by a special resistance wire wound upon it and the temperature was controlled by a thermo-element and resistance, so that the rate of rise of the temperature on each lot of brushes was at the rate of about 5° C. per hour over a period of about one hundred hours. The distillation of the binder under this treatment does not rupture or weaken the brush.

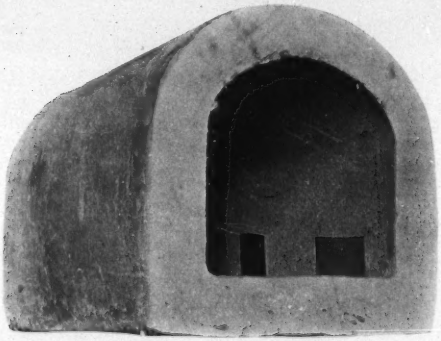
FIG. 3.

Muffle boxes and saggers.

When larger volumes of brushes are heated or the heating of small containers in a large furnace of usual type is carried out, the brushes near the walls receive a very different heat treatment from those near the centre of the furnace. This causes much irregularity of product, so that we still give all brushes the first firing or baking in the small wire-wound and heat-insulated muffles, of which Fig. 4 is an illustration. Fig. 5 shows a double row of these muffles with their control resistances. These latter

enable the operator to gradually raise the temperature in each muffle. The rate of this rise is learned by means of pyrometer couples inserted into the muffles. In this, which is a preliminary heating only, the temperature rises after one hundred hours to 500° C. In this process the volatile products of distillation have been so slowly eliminated from the brush that no striæ, laminations, splits, or blisters are produced. The brush is now bound together by a material largely carbon, but still capable of further shrinkage, hardening, and decomposition by higher temperature.

FIG. 4.



Muffle.

After the preliminary heating at 500° C. the brushes are unpacked and a series of tests would show relatively inferior qualities throughout, about as follows, for one type of railway motor brushes:

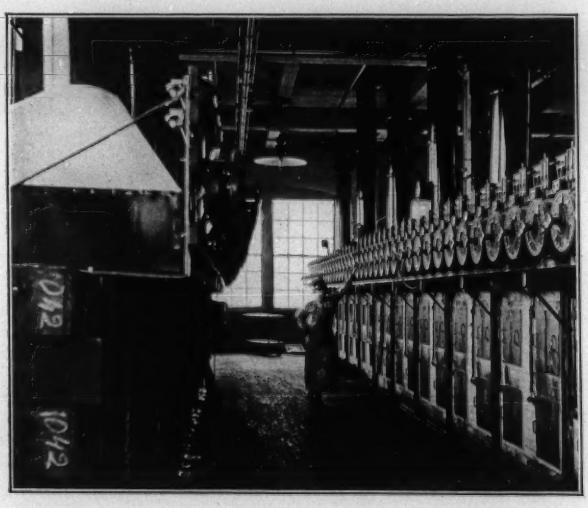
| Hardness. | Tensile strength. | Resistance. |
|---|---|---|
| 20 | 9000 pounds per square inch. | 0.043 ohm per inch cube. |

The material can still shrink greatly and be improved by a higher heat treatment. This is accomplished in an ordinary porcelain baking-kiln at about 1400° C., the brushes being packed as before, though not in iron, but in fire clay containers or saggers (at right in Fig. 3), the whole being covered with coke dust and the cover carefully luted on. The slightest carelessness in this

protection-process causes a softening of the outer surface of the brush, where a little combustion has removed the binder-carbon. This binder-carbon is easily combustible in the presence of an excess of coke powder.

Owing to the severe requirements for railway motor brushes, the work for a long time was devoted exclusively to this field, and there is probably still plenty of improvement possible.

FIG. 5.



Electrically heated muffles.

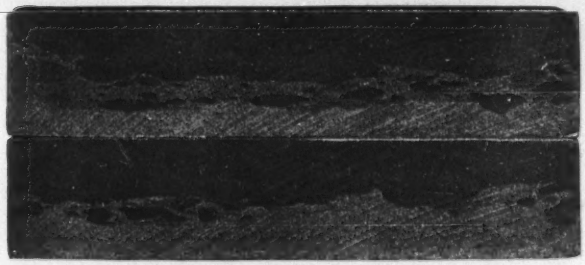
As the work has advanced, the refinements possible have become more and more apparent, and it is quite evident now that several different carbon brush types are necessary to satisfy the requirements for different types of machines. In case of this particular brush, however, it is worth attention that by slight modifications in the process, such as fineness of grinding, pressure on the hydraulic press, etc., the record of tests kept during the past two years shows the following changes. Attempts have been made to operate without change in the components,

as weighed out. These qualities have all improved through small refinements.

| Year. | Hardness. | Resistance. | Tensile strength. |
|---|---|---|---|
| 1909. . . | 52.3 | .00135 | 1800 |
| 1911. . . | 58-60 | .00120 | 2700 |

Development of the railway brush led to trying the same product on other types of electrical apparatus, and it was at once evident that the general brush requirements call for more than one kind of brush and more than a single composition. Generator brushes, while they do not meet the severe conditions met by railway motor brushes, and may therefore be softer and of lower physical tests, should have higher conductivity and should,

FIG. 6.



Sections of compound brush

in use, develop a polished commutator without cutting or smutting the metal. A fairly satisfactory type of generator brush may be made almost entirely of ash-free graphite and binder-carbon and will have approximately the following values on test: Hardness 35, resistance .00078, tensile strength 2500, chip test 6. It has been found that a brush needs some lubrication qualities which are difficult to quantitatively express. In the past it has even been customary to treat some brushes with oils, vaseline, etc., to give them this lubricating effect. It has also been found possible to improve operation of a commutator by using hard, strong, non-lubricating brushes and to interpose in several of the brush-holders a pure graphite brush, which serves to give desired lubrication for the other brushes. This has led to experiments

on a combination brush, of which a section is shown, Fig. 6. In this case, by a proper selection of proportions of binder or pitch and the mixture, it has become possible to gain the same shrinkage in the two widely different materials, a hard brush body and a body largely composed of graphite, so that the two are still firmly bound together after firing. A sort of laminated brush is thereby produced, and if there were service demands for such laminated brushes they could be produced.

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**Magnetic Properties of Iron under High Frequency.** E. F. W. ALEXANDERSON. (*Electro tech. Zeits.*, xxxii, 1078.)—Tests were conducted with an electrical high-frequency machine giving periods up to 200,000 per second. It is shown (1) that iron is able to follow so high a frequency; (2) that the permeability of iron under high frequency is most probably the same as with low frequency; (3) that the skin effect with a sheet 0.08 mm. thick is sufficiently great to prevent a higher induction than about B = 1200 being obtained with the highest frequency; and (4) that the iron used in the building of high-frequency motors might be reduced with advantage. Details are given of the apparatus and method, also tables and diagrams showing the results.

**Effect of Atmosphere on Wireless Telegraphy Sender and Receiver.** A. ESAU. (*Phys. Zeits.*, xii, 798.)—The effect of the condition of the atmosphere upon the damping of a wireless telegraphy sender and receiver is investigated. When the antenna wires are covered with hoar-frost or ice the damping is increased up to 200 per cent. In rain it increases up to 100 per cent., and when snow is falling up to 50 per cent. Fog causes very little change. The electrical condition of the atmosphere causes changes up to about 20 per cent. Atmospheric disturbances increase with the increasing transparency of the air, with decreasing moisture, with increasing wind velocity, and with the formation of thunder clouds. They diminish with increasing cloudiness, mist, and fog. They are more frequent at midday and at midnight. The maxima disturbances occur in June and August, with a secondary maximum in December. The winter months are quietest.

**Thulium.** C. A. v. WELSBACH. (*Zeitsch. Anorg. Chemie*, lxxi, 4.)—From a spectroscopic investigation of thulium preparations it appears that thulium consists of three elements, to which the symbols TuI, TuII and TuIII are assigned. Each gives a characteristic absorption and spark spectrum, which are described. It is possible that other elements lie between aldebaranium and TuI, and between TuII and TuIII.

# ELEMENTS OF THEORETICAL AËROMECHANICS.

## Part II—Aërodynamics.

BY
A. F. ZAHM, Ph.D.

AËRODYNAMICS may be defined broadly as the science of motion of air or an aëriform fluid. Commonly, air alone is implied in the word. This is especially true when the name is used by engineers. With them it is the analogue of hydraulics, which is the science of motion of water. Both sciences treat of not only the motion of their peculiar media, but also of its effect on objects or machinery connected with the fluids.

An important function of theoretical aërodynamics is to determine the velocity and stress of a fluid at every point of the medium when it flows past an obstacle, the physical properties and conditions of the fluid being assumed or given. From the point-velocity the stream-lines may be mapped; from the point-stress about an object the resultant pressure and friction may be found by summing over its surface. Equivalent results may be obtained if the object is assumed to move against the fluid, since only the relative motion is of consequence. This is regarded as self-evident.

It will be convenient to treat of aërodynamics together with the general science of fluid dynamics.

Theoretical fluid dynamics, being a difficult subject, is, for convenience, commonly divided into two branches, one treating of frictionless or perfect fluids, the other treating of viscous or imperfect fluids. The frictionless fluid has no existence in nature, but is hypothecated by mathematicians in order to facilitate the investigation of important laws and principles that may be approximately true of viscous or natural fluids. We may first notice some of the principles and conclusions in this easier branch, then pass to the treatment of viscous media.

### DYNAMICS OF FRICTIONLESS FLUIDS.

The motion of a fluid may be rotational or irrotational, and each of these kinds of flow may be steady or unsteady. The motion of the medium is irrotational if every part of it moves

with fixed orientation, so that if a small sphere of it were suddenly frozen it would drift along without rotating in any direction; otherwise the flow is rotational. The motion of the fluid is steady when its velocity at every point of the stream is constant in magnitude and direction.

In all kinds of movement the translational velocity of the fluid may be everywhere manifested by means of stream-lines; that is, lines drawn everywhere in the medium in the direction of flow of the coincident fluid. Each particle of the fluid, therefore, moves in the direction of the instantaneous stream-line passing through it. Evidently such lines are stationary in steady motion, but shifting from instant to instant in unsteady motion. In the latter case they are called lines of flow by some writers. In like manner lines may be drawn everywhere through the fluid, showing the direction of rotation of its small parts, and serving, so to speak, as axes of rotation for the fluid particles strung along them. These are called vortex-lines. In this text, however, none but irrotational motion of the fluid, or body immersed therein, shall have consideration.

The stream-lines are of great importance in the science of fluid dynamics and its practical applications to the flow of air and water. They not only mark the direction of flow at every point of the medium, but also fairly indicate the translational velocity of the fluid at each point, particularly if there be little change of bulk of the flowing particles; that is, slight expansion or contraction. A simple example will illustrate this.

Suppose a pipe of varying cross-section to convey in steady flow a liquid, or a gas, at practically constant density. Then, since the same amount of fluid per unit time passes through all segments of the pipe, evidently the flow is swifter through the narrow sections than through the broad, the average velocity at each part of the pipe being inversely as the cross-sectional area of that part. But if various stream-lines are drawn in the current they will crowd together in the narrow parts of the pipe and separate in the wider parts, thus showing the relative speed of flow from point to point, as well as the direction. Furthermore, stream-tubes may be drawn in the current, their walls being composed of stream-lines, or, say, delineated by them; and at every part of any such stream-tube, supposed very narrow, the velocity of flow is inversely as the sectional area of the

tube at that part. But since the longitudinal sections of the stream-tubes portray the stream-lines, these latter may in many cases fairly indicate the velocity of the current, being near together when the current is swifter, and *vice versa*.

Now, as any given particle, or small mass of fluid, moves along stream, its bulk, pressure, and level may change, as well as its velocity. Thus work may be spent upon the particle, both by gravity, in accelerating it, and by the surrounding fluid pressure, accelerating or compressing it. Hence, in case little or no compression occurs, *the work of gravity and pressure together equal the kinetic energy communicated to the small mass of frictionless fluid, in speeding it along its stream-line in the steady current*. This is Bernouilli's theorem[1] on the flow of liquids, here proved on the assumption that a particle of fluid can have an increase of its total energy only from external agencies, and by no means of its own; or, in other words, that a material system cannot increase its own energy, which is but a statement of the well-known principle of the conservation of energy.

Bernouilli's theorem is much used in the practical applications of fluid dynamics in science and engineering, both in its more general form, as above, and in simpler forms. Some of these will follow by way of illustration. But first we may notice the quantitative relation between speed and pressure at any point along a stream-line.

In case of horizontal flow the particle is accelerated along stream solely by the difference of fluid pressure on its front and rear. Thus it gains speed in the direction of waning pressure, so that where the pressure is least the speed is greatest, and the stream-lines are closest together. This obviously is true also of down-sloping stream-lines, since here the component of gravity aids in speeding the particle. It is true in up-sloping stream-lines if the fall in pressure more than compensates the retarding influence of gravity. Hence, in steady motion the stream-lines, besides accurately indicating the velocity and direction of flow, fairly indicate also the pressure.

The stream-line manifestations are very instructive where they can be practically observed, either on a diagram or in a liquid or gaseous current rendered visible by means of pivoted

[1] Daniel Bernouilli, *Hydrodynamica Argentorati*, 1738.

vanes or drifting particles. Where, for example, a current flows squarely against a disc well submerged, the stream-lines bend symmetrically round the disc, crowding together toward its periphery, but widening near the centre of the disc, thus disclosing the fact that the unit pressure is greatest at the centre of the disc and least at the periphery. Again, if a fluid flow from one large chamber to another, through a narrow straight, the stream-lines crowd together in the passage, showing increased velocity and diminished pressure.

To find the quantitative relation between the pressure, depth, and speed, in a frictionless fluid of given density, we may equate the gain of kinetic energy of a small unit cube of the fluid to the work done upon it by gravity, plus the work done by the difference of pressure on its front and rear faces in accelerating it along stream. Taking each in turn, we note, first, that the gain of kinetic energy of the unit cube equals one-half its mass times the gain of its velocity square; secondly, that the work of gravity equals the weight of the cube times its change of level; thirdly, that the work of the difference of pressure on the front and rear of the cube, in pushing it from one point to any other point in its path, equals the difference of hydrostatic pressure at these two points. To prove this third affirmation observe that while the unit cube moves once its own length the pressure-work done upon it equals the difference of pressure fore and aft multiplied by one unit length of displacement; then, as it moves again its own length, the work done equals the new pressure difference multiplied by unit length of displacement, and so on as the small unit volume traverses successive lengths of itself along any segment of the stream-line. Summing all these successive products of the pressure difference by the common unit length gives simply the total pressure difference between the first and last position of the moving particle, as a measure of the work spent by the pressure difference on the front and rear faces of the unit cube in speeding it along stream and increasing its kinetic energy. This is obviously true when the cube of incompressible fluid changes length in moving along the stream-line; for a change of length means a corresponding change of cross-section, thus leaving the resultant pressure difference on its front and rear faces the same, assuming the pressure gradient along stream to be finite. Equating the first work-

quantity to the second plus the third, gives the analytical relation sought. This relation may obviously be generalized to cover elastic media, by writing the work of pressure and gravity equal to the gain of kinetic energy plus the work of compression.

In order to translate this verbally expressed relation into a symbolic formula, or equation, let $\rho$ be the constant mass of the fluid particle, taken as a unit cube; $z_1$, $z$, its initial and final distance above any fixed level; $p_1$, $p$, the initial and final pressure on its face; $v_1$, $v$, its initial and final velocity, as it traverses any segment of a stream-line. Then the above verbal expression generalized may obviously be written:

$$(p_1-p) + g\rho\,(z_1-z) = \tfrac{1}{2}\,(v^2-v_1^2)$$

in which the first term is the difference of pressure at the ends of the assumed course of the particle; the second term is the work of gravity upon the particle, being its weight times its change of level; the third term is the gain in kinetic energy, being one-half the mass times the increment of the velocity square. This equation is Bernouilli's analytical expression connecting $p$, $z$, $v$, $\rho$, the variable pressure, depth, and speed of a fluid particle, the other symbols representing fixed and given quantities. A few simple examples will show its usefulness, beginning with incompressible fluids, for which the fourth term of the equation is omitted, being zero.

Let it be required to find the speed in a horizontal stream of water, or of air, at practically constant density, starting from rest in a chamber at constant pressure $p_1$, and flowing steadily through a narrow channel into another chamber at constant pressure $p$. Since the fluid starts from rest $v_1 = o$, and since the flow is level $z_1 = z$; thus Bernouilli's equation gives $v^2 = 2\,(p_1 - p)/\rho$. As, in this expression, all the quantities except $v$ are given, the velocity is determined.

In case the initial pressure, in the foregoing example, is due solely to the weight of the fluid, as in a large tank of liquid in vacuo, if $h$ be the depth of the narrow straight or aperture, then $p_1 = g\ \rho\ h$; and if the flow ends in vacuo, $p = o$; so that Bernouilli's equation for this simple case is $v^2 = 2\ g\ h$. This value of the velocity of discharge is the same as that of a body falling freely through the height $h$; and, if the discharging

stream be directed upward, the fluid will rise to the level of its free surface. This interesting relation, deduced so simply from Bernouilli's theorem, as a very specialized case, was first discovered experimentally by the philosopher Torricelli, and is commonly known as Torricelli's theorem, published by him in 1643.

The above deductions from pure theory—that is, from the mathematical analysis of the flow of a frictionless, or hypothetical, fluid—have been tested experimentally, and found to give satisfactorily the velocity of such natural fluids as air and water under simple conditions. The writer[2] in 1902, and Eiffel in 1909, made a careful test for the first medium, by measuring the speed of influx of air from a room into a chamber at reduced pressure and at all speeds up to more than twenty-five miles an hour, and found the computed speeds to agree closely with the observed ones. The speed of efflux of liquids has been carefully determined many times since the day of Torricelli, sufficiently corroborating his conclusions for all but extreme conditions. It may be said that, for both cases cited, the speed of efflux is slightly less than for a perfect fluid—commonly one or two per cent. less—owing to the loss of kinetic energy by friction as the fluid particles rub the channel wall, or crowd and deform each other, thereby doing internal work. Of course, for long and tortuous passages the formula is inapplicable to natural fluids, because of friction.

It is important, also, to note that the argument by which Bernouilli's theorem was deduced may be worded so as to apply to a fluid mass spending its kinetic energy in overcoming gravity and the difference of pressure, front and rear, as it flows along stream to points of higher level and pressure. While the wording will be reversed, the symbolic expression, or equation, will remain unaltered, since the symbols as they stand are perfectly general, and apply to both accelerated and retarded flow. This generality adds to the usefulness of Bernouilli's theorem.

Suppose, for example, that a horizontal jet or current of air or water strikes squarely against a flat surface, and it is required to find the unit pressure of the fluid at the centre of impact. At this point the speed $v = o$, while in the unchecked stream $p_1 = O$;

[2] "Measurement of Air Velocity and Pressure," *Physical Review*, December, 1903.

and everywhere $z = z_1$, so that Bernouilli's equation gives $p = \rho\, v^2/2$ as the pressure at the centre of impact. Thus, if it struck into a hole in a tank, it would, since $p = \rho\, g\, h$, balance a fluid practically of the height $h = v^2/2g$ of the fluid above the aperture, or the "height due to the velocity."[3] This has been proved experimentally. As an instance, the reader may be referred to Mr. W. M. White's paper, "The Pitot Tube: Its Formula," published by the Association of Engineering Societies, under date of August, 1901.

The formula $p = \rho\, v^2/2$, found for central impactual fluid pressure, is made the basis of the pressure tube anemometer and hydrometer, instruments for measuring the speed of flow of air or water, and commonly called the Pitot tube, after Pitot, who first devised it. If an open-mouthed tube is held along stream—in water, for example—so as to receive the full impact of the current, and if the resulting increment of pressure in the tube is measured, the velocity can be accurately computed from the foregoing formula $v^2 = 2\, g\, h = 2p/\rho$. In like manner, if the excess of pressure of the wind blowing squarely into the hole, as compared with the pressure in the unchecked wind, is observed, the wind-speed can at once be calculated by the same formula.

Many other examples may be proposed and worked out to illustrate the use of Bernouilli's equation for the steady motion of a frictionless incompressible fluid, and its application to the flow of natural liquids and gases, under conditions involving only slight friction and change of density; but these may be left to the reader's fancy or requirements. Further treatment, also, of these interesting questions may be found in any thorough book on hydrodynamics, such as the elegant English treatise of Prof. Horace Lamb.

When considerable compression or expansion occurs in the moving fluid, Bernouilli's equation may still be applied, but only in its more generalized form containing a fourth term, which expresses the work done in altering the bulk of the unit cube of the fluid. In practice the equation is divided by $\rho$, so that the fourth term is the work done in compressing the volume of unit mass of the fluid. But this change of bulk may follow any one

[3] The height due to the velocity is the height to which a free body rises when started vertically upward with the given velocity.

of a myriad laws of expansion. Usually some law is given or assumed. Thus, the expansion or compression may occur at constant pressure, constant temperature, constant heat content. If the pressure is constant the work of compression is obviously $p\ (V - V_1)$, or the pressure times the change of volume of the unit mass; and hence this expression, or its equivalent $p\ (1/\rho - 1/\rho_1)$, must be used as the fourth term in Bernouilli's more general equation. If the flow occurs at constant temperature the work of compression is $R\ T\ log\ (p/\rho_1)$ in which $T$ is the absolute temperature, and $R$ is a constant for the fluid. This expression, as also the one for the work done in adiabatic compression, may be found duly derived in any adequate treatise on heat-motors, or on thermodynamics or hydromechanics. The formulæ need only be referred to here, being too abstruse for the present treatment.

Thus far we have studied the motion of fluids by aid of the principle of the conservation of energy, and by its means have found the relation between the pressure, velocity, and density at all points of the moving fluid, as expressed in Bernouilli's theorem. In like manner we may obtain many useful results from another fundamental principle of physics, known as the principle of the conservation of momentum.

Broadly stated, this principle asserts that a system of bodies cannot change their aggregate momentum by mutual action. For example, a gun throws a projectile northward, and is itself hurled southward with equal and opposite gain of momentum. The earth draws an apple downward, and is itself drawn upward. The impulse of the one is equal to and opposite the impulse of the other, being measured by the product of the mass into its gain of velocity. In other words, the velocities gained by propellant and propelled are inversely as their masses. Furthermore, since the gain of momentum per second is a measure of the force causing it, propellant and propelled are urged apart with equal and opposite forces. This much for linear motion; for angular motion a like relation obtains. The angular momentum per second communicated is equal and opposite to that received, and is a measure of the angular force, or torque, causing the angular velocity. What has been said of the generation of linear and angular momenta applies also to their destruction. It takes a given force as long to stop a certain free motion as to start it.

Many practical problems in fluid dynamics may be solved directly by use of the foregoing principle. If, for example, a jet of air or water issues from a tank, its reaction, or back pressure, on the tank equals the momentum per second communicated by the tank to the jet, or the mass per second times the velocity. Likewise, when the jet strikes squarely on a plane surface and flattens out, losing its forward velocity, it exerts a pressure equal to the mass per second of the impinging fluid multiplied by its speed. Thus if $\rho$, $a$, $v$, be the density, sectional area, and velocity of a jet, its reaction against the tank ejecting it, and the pressure against a normal plane stopping it, each equals in numerical magnitude $\rho a v^2$, this being the product of the velocity of the impinging jet multiplied by the mass per second, $\rho a v$, of fluid delivered by it.

In particular, if the jet has a cross-sectional area of one square unit, its whole impactual pressure against the plate will be $\rho v^2$, providing the plate be broad enough to stop the forward velocity. This total pressure of the flattening jet is just twice the unit pressure at the centre of impact as previously calculated. Hence, if the plate be pressed against a unit orifice in a tank, it will support a fluid column of twice the "height due to the velocity," or twice the height of the source. Putting it numerically, if a one-inch stream of water, from a tank of ten feet head, strikes squarely against a flat plate stopping a one-inch hole in a second water tank, it will sustain in the second one a head of nearly twenty feet. This phenomenon may be verified experimentally, and is sometimes called the "hydraulic paradox"; because, while the back pressure, or reaction, of the stream against the first tank equals the impact pressure against the second, the water level is twice as high in the second as in the first. The puzzle is resolved by noting that the impact on the plate is sustained by just one square inch of static fluid surface in the second tank, while in the first tank, or source, the issuing one-inch jet exerts its back pressure against a static fluid surface of more than one square inch, as may be seen by tracing the jet back into its source to where it comes practically to rest. The phenomena may be varied by curving the periphery of the plate so as to make the stream rebound, thus still further increasing the impactual pressure, and heightening the fluid in the second tank. Obviously, the fluid in the second tank may be

sustained at any height whatever, by force of the given jet, if the given plate be made to close an aperture sufficiently small. But in all cases it must be noted that, as formely calculated, the unit pressure at the centre of impact equals the pressure of the source, so that if a pin-hole be made there in the plate, the level in the second tank cannot exceed the level of the source.

What has been said of a steady horizontal jet issuing from a reservoir may be affirmed also of a stream hurled from a propeller. Its reaction on the propeller equals the impulse of the stream against a broad normal plane which just stops its forward velocity; while both reaction and impulse severally equal, in numerical value, the change of momentum per second required to maintain or stop the propeller stream. This statement, of course, is true of all propellers. But in applying the principle to a practical case there is need of precaution. For if a closed circulation occur, so that the fluid issuing from the rear of the propeller gradually returns to its front and passes through without being started from rest, or practically so, the formula derived for the jet is not precisely applicable, but gives an excessive thrust. In a small, clear room the circulation may be so free as to make the thrust less than half that it would be in the open.

When the momentum principle is employed, the propeller thrust may be written, $R = m\,v$, in which $m$ is the mass of fluid per second set in motion, and $v$ the velocity imparted. This shows that the propeller thrust increases directly as the velocity imparted per second to a given mass of fluid. Accordingly, aëronauts sometimes think it advisable to hurl the air with immense speed. They might not be so far wrong if they had unlimited engine power free of cost. But the formula also shows that a large thrust may be had by hurling a large stream at slow velocity.

The principle of the conservation of momentum applies in like manner when a stream, instead of being started or stopped, is merely diverted. If a jet of velocity $v$, striking an oblique object, like the curved blade of a turbine or water-wheel, suffers a loss of forward velocity represented by $v_1$, and acquires a lateral component of velocity $v_2$, it exerts on the object a forward impulsive pressure $\varsigma\, a\, v.v_1$, and a lateral pressure $\varsigma\, a\, v.v_2$, $a$ being the cross-sectional area of the jet; while the entire impactual force against the diverting surface is the

resultant of these two rectangular components. Obviously, if $\propto$ is the angular deviation of the jet, its lateral gain of velocity is $v_2 = v \sin \propto$, and its loss of forward velocity is $v_1 = v(1 - \cos \propto)$, this latter being the difference between the velocity before impact and the forward component of velocity after impact; and these values of $v_1$, $v_2$, substituted in the foregoing equations, give the rectangular components of the impulsive pressure of the jet.

As a practical application of these formulæ, if the stream from a propeller encounters a large aëroplane surface, or sail arched downward, it will exert a lift on the sail whose magnitude equals the down-increment of momentum per second, and this is evidently equal to the whole momentum per second of the propeller stream multiplied by the sine of the angle of deviation. The maximum lift so obtainable is therefore not greater than the propeller thrust, and this occurs when the stream is turned straight downward with unabated velocity. At the same time the drift, or rearward pressure on the aëroplane surface, approximately equals the propeller thrust multiplied by one minus the cosine of the angle of deviation of the stream, as above explained. These relations should have due consideration by those inventors who propose to lift an aëroplane vertically in space by directing the propeller stream against its wings. Doubtless the feat is physically possible, but it requires far more power than to support the same weight in the customary way, unless the wings be so shaped and so near the earth as practically to imprison the air beneath them. In this latter case a small motor could lift an indefinitely heavy aëroplane.

If the jet impinge on a flat plane of inclination $\propto$ and be diverted by this amount, the change of velocity normal to the plane is obviously $v \sin \propto$ and the consequent impactual unit pressure is $\rho v^2 \sin \propto$

So much for the theory of jets, or narrow fluid streams. One naturally inquires what is the whole impactual pressure on a body immersed in a very large stream like the wind. It must be said that no satisfactory general analysis of the resistance of surfaces submerged in natural fluids, under ordinary conditions, has yet been worked out, so that the only reliable resistance values are those obtained by cautious and skilful experimentation. For a fluid, however, composed of small discrete particles widely separated, whose impact occurs without mutual influence, the

pressure can be computed by the simple method employed for the narrow jet striking a broad plate. Such a fluid was hypothecated by Sir Isaac Newton, and yielded formulæ which may be fairly true for very rarefied gases, but which in general are far from true for fluids under ordinary pressures.

In the Newtonian analysis each filament of the fluid stream is assumed to encounter the submerged obstacle uninfluenced by any other filament. If the impact is perfectly inelastic, the particles lodge or flatten out on the submerged obstacle; if it is perfectly elastic they rebound with unabated speed, but with altered direction. In either case the whole pressure of impact, and the exact stream paths, can be readily calculated for any given form of geometrical surface by treating each filament separately and summing the effects of all.

Consider a submerged plane surface. If a plane is set normally in such a current it sustains, on each square unit of its surface, a pressure equal to $\rho v^2$, since each unit of surface is bombarded by an independent jet, delivering $\rho v$ units of fluid mass per second against the plane, and each unit mass suffers a change of velocity $v$, assuming perfect inelasticity. The whole resistance of the stream is therefore $\rho a v^2$, $a$ being the area of the plane, which expression has the same form as that for a single fluid jet. If the plane is more and more inclined, fewer filaments strike, and each with an impact softened by the obliquity; but the impactual pressure is easily computed by taking account of the angular deviation, as explained for a single jet. For perfectly elastic impact the resistance is, of course, twice that for inelastic impact, since the change of velocity component normal to the plane is twice as great for elastic as for inelastic impact, the component velocity parallel to the plane having evidently no influence on the impactual pressure, since it remains unaltered and occasions no change of momentum in the impinging stream. Thus for a plane of area $a$ and inclination $\propto$ the sectional area of the intercepted stream is $a\sin\propto$, the unit pressure for inelastic impact is $\rho v^2 \sin\propto$, and the whole pressure is $\rho a v^2 \sin^2\propto$.

In a similar way the flow and resistance may be found for spheres, cones, ellipsoids, and other geometrical forms. In all these it will be observed that the particles strike only the front surface, where they lodge or else rebound, then speed away without touching the rear or exerting pressure upon it. Such

effect may occur in the highly-rarefied atmospheres of planets when struck by meteorites, and less fully in the motions of very high-speed projectiles, which are immediately followed by a rarefied and feeble wake. In general, if the rear pressure can be neglected, the resistance of a given object can be said to vary directly as the square of the velocity; for it is measured by the mass per second of fluid disturbed times the velocity of disturbance, thus involving the velocity to the second power, as in the case of the normal plane above considered.

A numerical value of the atmospheric resistance to a normal plane may be derived from the foregoing formula $\rho\ a\ v^2$, and is of some historical interest. Writing the density $\rho$, of the air equal to one-thirteenth of a pound per cubic foot, and $a$ equal to one square foot, the pressure per square foot becomes $v^2/13$ poundals, in which $v$ is the velocity in feet per second. Expressed in pounds force and miles per hour, the resistance per square foot is easily found to be $v^2/200$. This value is sometimes given in text-books, and is said to have been found experimentally by certain early investigators, but is now known, from more accurate measurements, to be some fifty per cent. greater than the true value. In passing it may be observed that the above formula is valid in case the normal plane be surrounded by a sufficiently broad guard-ring and the rear pressure remain constant, as when a vertical stream of water falls on a level plate.

Further treatment, both theoretical and numerical, as well as experimental, of the resistance of submerged objects having impactual pressure solely on the front surface, may be found in Newton's *Principia*. For the analytical discussion, illustrated by numerical computation and compared with experiment, giving the resistance of bodies when the impactual fluid pressures on both front and rear are taken into account, the reader may refer to Colonel Duchemin's work[4] on the laws of resistance of fluids. His general equation for the resultant impactual pressure on any blunt body, or any plane surface moving normally through the air, is, for ordinary speeds of transportation, $k\ s\ a\ v^2$, in which $k$ is a numerical constant for the given surface, and is known as the coefficient of resistance, or constant of figure. For higher speeds he obtains a formula involving both the square and cube of the speed.

[4] *Lois de la Resistance des Fluids.*

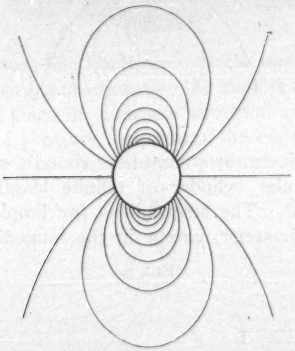
We now have touched lightly on some of the most elementary topics in the extensive, not to say formidable, science of fluid dynamics. Fortunately, they are also among the most interesting topics, and the most valuable for practical use. By the aid of analytical machinery too elaborate for the scope of this work, other principles and conclusions may be derived, at once entertaining and important when duly applied. But it will be sufficient merely to state, without proof, some well-established theorems on the flow and resistance of a perfect fluid about various familiar geometrical forms.

In general it may be said that a frictionless incompressible fluid of infinite extent, flowing steadily and irrotationally past a body of whatever shape, encounters no resistance, and exerts no resultant pressure upon the body, but presses it equally on both front and rear. This is true not only of fair shaped objects, like the torpedo and the two-edged sword, but of angular and flat forms as well, such as the cube and normal plane. The proposition has been affirmed also of natural fluids flowing past objects of easy shape, by certain well-known writers, assuming the surface friction of such bodies to be negligible. But the proposition is untrue in theory, and disproved by experiment. Theory shows that no viscous fluid can flow without resistance past any geometric solid, even though perfectly smooth; and experiment shows that all such bodies offer both head resistance and skin-friction. It is evident without computation that viscous fluids must encounter some resistance in flowing about solids, however smooth, because the stream-lines crowd each other, and adjacent fluid filaments run at different speeds, thus generating internal friction and consuming energy, which can be derived only from the resultant pressure of the immersed object. It may be inferred, however, that when the motion is so extremely slow that the frictional effect is negligible the viscous fluid behaves sensibly like a perfect one.

Another general theorem asserts that when the above hypothecated frictionless fluid moves past a solid with accelerated speed, or when the solid moves with accelerated speed through the quiescent fluid, there is always a resistance, and this is directly proportional to the acceleration. Commonly the fluid resistance, in accelerated motion, can be expressed by merely endowing the moving object with a certain additional mass, and writing its impulsive resistance equal to the product of this additional mass

multiplied by its acceleration; entirely ignoring the pressure of the fluid. For a sphere the additive mass is half that of the displaced fluid; for a circular disc moving normally it is slightly over five-eighths of the fluid mass displaced by a sphere of equal diameter; for a circular cylinder moving transversely it is exactly the mass of the fluid displaced; for an elliptic cylinder it is the same as for a circular one whose diameter equals the major or minor axis, whichever is placed transversely to the line of motion. In each case the additive mass of the moving object multiplied by the acceleration gives the fluid resistance; but, of course, to find the whole resistance to acceleration, due to the inertia of both the body and the medium, the mass of the

FIG. 1.



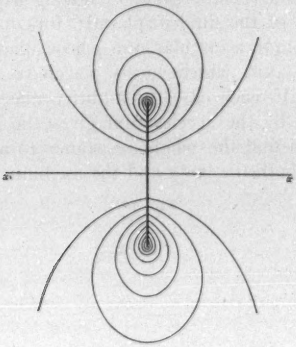
body itself must be combined with the fictitious additive mass, which latter represents only the equivalent of fluid resistance. For example, the resistance of a sphere is $(m + m')\, j$, in which $m$ is the mass of the sphere, $m'$ is half the mass of the fluid displaced, and $j$ is the acceleration of the moving sphere.

Enough has been said to show that the equivalent inertia of a solid accelerating through a fluid is very material, if its density and that of the fluid are at all comparable. Due account of this fact, therefore, should be taken, in studying the resistance of natural fluids in unsteady motion, or of objects accelerating through such fluids. Unfortunately, this is not always done, even by fastidious experimentalists.

The stream-lines of the hypothetical fluid under consideration,

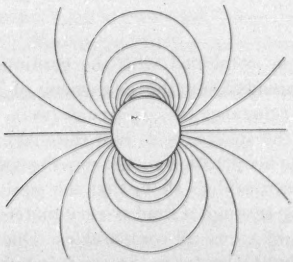
as it flows steadily past opposing objects, have been determined mathematically for various geometrical forms, and graphically outlined for a number of interesting cases. Figs. 1, 2 and 3

FIG. 2.



represent respectively the stream-lines round a sphere, a circular disc, and a circular cylinder of infinite length, each moving through the fluid. The stream-lines for limpid natural fluids are of similar character, except in the immediate rear of the

FIG. 3.

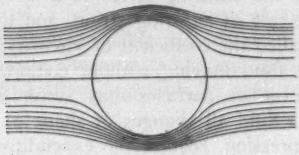


moving objects, where a turbulent wake occurs, and at sharp turns and angles which produce eddies on the sides and rear. Also it may be noted that for elastic fluids the maximum de-

flection of the outer stream-lines cannot occur opposite the widest section of the obstacle, but occurs farther to the rear, some time being required to carry the impulse outward. This will be seen in the stream-line figures obtained experimentally.

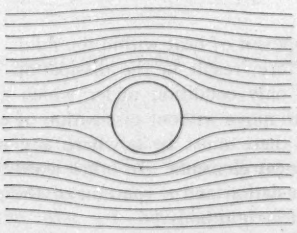
The theoretical stream-lines for a sphere, a circular cylinder, and an inclined plane, each held stationary in the medium, are shown in Figs. 4, 5 and 6. In both front and rear of each there

FIG. 4.



is a median stream-line abutting squarely upon it at the point of greatest pressure, and separating the main parts of the current. For the inclined plane the points of abutment are not, in general, at the centres of pressure, but nearer the edges than these are. Needless to say that the resultant pressure is in front of the

FIG. 5.



centre of surface, so that an inclined plane tends to set itself squarely to the current, if pivoted at the centre of surface.

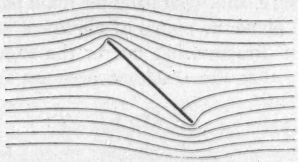
## DYNAMICS OF VISCOUS FLUIDS.

The general equations of motion of a viscous fluid have the same form as those representing the motion of a frictionless fluid, except for the addition of terms involving the coefficient of

viscosity. This coefficient represents a physical quantity which has been accurately determined for many fluids by exact measurement. It is sometimes also termed the coefficient of internal friction. When the velocity varies from stratum to stratum in a viscous fluid, the unit shearing stress on the face of any thin lamina taken parallel to the flow, varies directly as the fluid velocity gradient taken squarely across the lamina. The ratio of this stress to the gradient is called the coefficient of viscosity. In other words, the shearing stress in a viscous fluid is proportional to the rate of shearing strain, and the factor of proportionality is called the coefficient of viscosity.

In a perfect fluid moving without compression no heat is generated, however its particles may suffer change of shape. But in a viscous fluid all changes of shape of the particles, as well as all compression, require the expenditure of work, and

FIG. 6.



cause the development of heat within the fluid. All such changes of shape, therefore, result in the dissipation of energy. This means that the only condition under which a viscous fluid of constant bulk can move without dissipation of energy by internal friction is that there must be nowhere any extension or contraction of its linear elements. In other words, the motion must consist of a translation and a rotation of the mass as a whole, as in the case of a rigid body.[5]

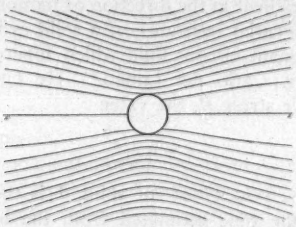
In all cases of motion, therefore, of a solid through a natural fluid, whether at constant or at variable velocity, resistance is encountered, and energy must be expended in propulsion. This is true even of an infinitely thin material plane moving edgewise, for if the fluid slips along the laminar surface it offers sliding resistance, while if it adheres to the lamina it still offers resistance due to the internal friction between the adherent layer

[5] Lamb, "Hydrodynamics," 1906, p. 541.

of fluid and the stratum adjacent, the whole neighboring part of the fluid suffering lamellar strain. Both cases of motion are of interest in aërodynamics, that in which the surface slip is inappreciable, and that in which it is pronounced.[6] The first condition is usually assumed in the analytical treatment of problems in viscous fluid dynamics. Of the many problems so treated only a few are of sufficient interest to be noticed here; and only the results of their solution can be expressed in elementary form. These may be briefly presented.

The resistance of a sphere translated at constant speed through an infinite incompressible viscous fluid has been determined for the case in which its radius and speed are so small as to make the inertia effect negligible by comparison with the

FIG. 7.



viscous effect. The expression found for the resistance takes the simple form:[7]

$$R = 6\pi\mu a U$$

in which $a$ is the radius of the sphere, $U$ is the velocity, and $\mu$ is the coefficient of viscosity. The stream-lines caused by the translation of the sphere are as shown in Fig. 7. The equation is applicable to the case of fine sand falling in water, or to fine mist or dust falling in air, but is not applicable to bullets or larger bodies, moving at a considerable rate of speed.

The rate of flow, $Q$, of a liquid through a tube, with no

[6] Some writers have questioned whether slipping occurs between a solid and fluid in relative motion; but the fact that fine snow-flakes slide rapidly over smooth ice, due to the wind, seems to manifest slipping between the air and ice. Marey's wind-tunnel experiments furnish like evidence of slipping between air and solid surfaces.

[7] Lamb, "Hydrodynamics," 1906 Ed., p. 553.

slipping at the wall, is given by the expression

$$Q = ca^4(p_1 - p_2)/l$$

in which $a$ is the radius of the pipe, $p_1 - p_2$ is the fall of pressure in the length $l$, and $c$ is a constant of the value $\pi/8\mu$. Thus the rate of flow through a tube varies as the fourth power of its diameter, as the pressure difference at its two ends, and inversely as its length. Since every quantity in this equation can be measured, except $c$, the expression may be used to find $c$, and thence the coefficient of viscosity. The equation exactly expresses the laws found experimentally by Poiseuille in his researches on the flow of water through capillary tubes.

For the case of accelerated motion of a viscous fluid the mass times the acceleration of a small element of a stratum in laminar flow may be equated to the resultant shearing stress on its faces, if the pressure gradient in the direction of motion be ignored. In this way the tangential resistance of a solid cylinder rotating in an extended stagnant viscous fluid which adheres to it has been computed by Dr. Carlo del Lungo.[8] He finds for the unit tangential surface stress $R$, the value

$$R = u\sqrt{\frac{\mu\delta}{\pi t}}$$

in which $u$ is the velocity of the solid, $\delta$ is the density of the fluid, and $t$ is the time estimated from the beginning of the rotation. Applying this result to compute the frictional resistance of a thin material plane of length $l$, width $a$, and area $S = al$, Lungo obtained for the total friction $F$, on one side of the plane

$$F = 2S\sqrt{\frac{\mu\delta u^3}{\pi l}}$$

which can be written more simply in the form

$$F = cu^{1.5}l^{0.5}$$

From this he concludes that the skin-friction of fair-shaped bodies should vary approximately as the $3/2$ power of the speed and the $\frac{1}{2}$ power of the length. A like equation obtained by a different mathematical process was published later by F. W. Lanchester.[9]

[8] *Sulla Resistenza D'Attrito fra Solidi e Liquidi, Revista Tecnica, Anno V, Fasciolo 6.*

[9] "Aërodynamics," London, 1908.

## THE PRACTICAL APPLICATION OF SCIENTIFIC MANAGEMENT TO RAILWAY OPERATION.

BY
WILSON E. SYMONS,
Chicago, Ill.

(*Discussion continued.*)

MR. FRANK B. GILBRETH.—It is exceedingly appropriate that the Franklin Institute and the American Society of Mechanical Engineers should hold a joint meeting at Philadelphia for the discussion of the practical application of Scientific Management to the operation of railways, because, first, the Franklin Institute has a long record of pioneer investigation in the great national problems; second, the important papers on management have been presented before and can be found in the *Transactions of the American Society of Mechanical Engineers;* and, third, Messrs. Taylor, Dodge, Day, Tabor, Hathaway, Lewis, and Cooke—pioneers in efficient management—are all Philadelphians.

There can be no question that American railroads are efficient, especially efficient from the standpoint of all that has usually been considered good railroad management; but, as has already been stated by me when I was subpœnaed before the Interstate Commerce Commission at Washington and Mr. Brandeis cross-questioned me, I believe that the railroads will, without a doubt, be much more efficient when they adopt the Taylor plan of Scientific Management.

Although I have personally been engaged in some work on railroads, I have not sufficient data on which to base an opinion as to the amount of money that it is possible for the railroads to save per day should they all adopt the Taylor plan of management. I have no knowledge of the Santa Fe, except as a passenger.

There is no reason to doubt that the railroads would be able to pay larger wages to all employees, and have still more earnings for dividends and improvements, if they should adopt the Taylor plan of management, and I feel sure they will adopt it sooner or later. The history of railroading in America shows

that the managers have not always adopted improvements at first sight, but that they have usually adopted the best procurable sooner or later.

Now, there are other factors that will delay the general adoption of Scientific Management by the railroads besides the time necessary to convince the railroad managers and their employees that the Taylor plan of management is not like any of the old, unfair, "bonus schemes," but is founded on the Golden Rule, and is the one plan that will raise wages and reduce costs simultaneously. One of the causes for delay of adoption is the great cost of the duplication of the work of establishing ultimate standards of Scientific Management, if all railroads were each to do that work independently, simultaneously.

It would seem that Mr. Symons, though he be recognized as a railroad expert, is not at all familiar with Scientific Management when he "disposes" (see page 13) of 16 classes of the 18 classes into which he divides railroad employees, and then apparently doubts the possibility of applying Scientific Management to the last two.

If the Taylor plan of Scientific Management is to be applied, it is for all and not for a few; for the greatest economy and efficiency it should apply to and govern all, from the highest of the managers to the lowest of the employees. We have applied it with a large increase of efficiency to office employees as well as to those doing manual labor.

Mr. Symons excludes locomotive repairs in the following words: "The character of repairs made necessary by the foregoing conditions renders it absolutely essential to successful operation that both the foreman in direct charge of each operation, and the mechanic who is actually performing the work, shall not only be *permitted* but *required* to bring into use their mechanical skill and judgment, which, under the proposed plan of 'scientific management,' they would be prevented from doing." There is nothing under the Taylor plan that does not apply to repairs. In fact, it would be but fair for anyone to concede that any man who has made the remarkable mechanical inventions that Dr. Taylor has made, who has had the remarkable opportunities of education, training, general and specialized experience, and travel that he has had, would never for a moment advocate any plan of management that did not apply to anything

so perfectly obvious as repair work. A little more than a moment's consideration will serve to remind anyone that the more difficult the problem confronted, and the more unexpected the conditions, and the more the problem differs from standard, the greater will be the necessity for the work to be planned by the man best fitted by experience and special training, assisted by the command and use of records of previously standardized elements of all previous similar jobs, to determine what shall be done, and to prescribe the exact quality of workmanship with which it shall be done.

The entire jobs may be very seldom alike, but the number of their elements which differ are, by comparison, very few.

Mr. Symons's fears, as shown in his statement: "Such a system would have a tendency to discourage both men and officers from that personal initiative essential to a *strong, efficient,* and *economical* organization," are not borne out in practice, as can be seen daily in the two most highly systematized shops in the world—the Tabor Manufacturing Company and the Link-Belt Company, both of which happen to be right here in Philadelphia.

In my own experience, I have never seen a case where a foreman or workman has had his personal initiative discouraged, but, on the contrary, I have seen hundreds of men delighted with the fairer treatment, higher wages, better conditions, and more definite recognition of their individual merits.

The recommendation of Mr. Symons on page 19 is achieved by Scientific Management better than in any other way, because the measured output, task, or piecework plan is determined by the one and only fair method, namely, time study, which is based upon scientifically derived fundamentals, makes allowance for the human element—the variations and differences of individuals—and makes due allowances for unavoidable delays and for overcoming fatigue. For example, the allowance for rest for overcoming fatigue is never less than 10 per cent., and, for some kinds of work, more than 50 per cent. of the entire working day.

Without time study, Mr. Symons's piecework prices can never be set "fair and just alike, first, to the men, and, second, to the company." Without time study it will ever be necessary for the worker to dawdle, and soldier, to protect his best inter-

ests (see professional papers Nos. 647, 928, 1003, of the American Society of Mechanical Engineers, also "Principles of Scientific Management," published by Harper & Bros.).

Without time study as a preliminary in standardizing elements and operations the "measured output" will be an unfair gauge to effort or to efficiency, because the workman cannot be furnished with continuous conditions such as affect the supply of materials, or conditions of machinery, or "working opportunity."

Our work, while not the operation of a railroad, is the handling of mechanics and laborers in many of the trades of building construction, and offers probably as many difficulties as does the handling of railroad employees. We have always succeeded in greatly reducing production costs and time of construction, and raising laborers' and mechanics' wages from 30 to 100 per cent. simultaneously by the installation of the Taylor system.

Mr. Symons's conclusions are not well founded, if he thinks that the Taylor plan of management entails putting men "unfamiliar with the practical operation of a railway" in charge of its management.

H. K. Hathaway, vice-president of the Tabor Manufacturing Company, one of the best known of the Taylor-trained managers, says, in *Engineering Magazine*, April, 1911, speaking of the men who should develop Scientific Management:

"These men should be selected from the existing force wherever possible, and in case they are not available, and it is necessary to bring in new men, they should be directly in the employ of the company, and not in that of the systematizer. This is almost imperative if the work done is to be of a permanent character. If new men must be brought in, it should be done some time before the active development of the system is started, so that they will have ample time to become familiar with the plant and its output, and get acquainted with the other employees. These men should be started as workmen."

The Taylor plan in no way proposes to eliminate the present efficient workers. It protects such as these, and insures them a better and more agreeable job. They are invariably in favor of the new way when they have, by personal experience, discovered that the fears that have been roused in them by those who

have not worked under the Taylor system, and have therefore led them to believe that it is simply a new scheme of the employers to flimflam the employees, are not well grounded.

There must be employed, however, at the beginning, and until the managers and functional foremen have been taught their new duties and until the system is self-perpetuating, scientific managers who can teach the foremen and the men their duties.

The difficulty of obtaining the right men with the proper training and knowledge, coupled with the time and expense of duplication of the work of collecting motion study, time study, and other data of the trades, analyzing the data, building up methods of least waste synthetically, establishing standards, reclassifying and dividing and assigning functions of management, and then converting and teaching the workmen: (1) That it is good for them, and (2) How to perform their new duties, makes it impracticable for a railroad or any other organization to do its own pioneer work.

There are no "non-productive men" unless those that "plan more than they perspire" are, on account of that fact, called non-productive.

All those who have investigated the conditions agree that railroad employees are not receiving as large wages as they should, and also that at present those best informed in the management of railroads claim that they cannot increase the employees' wages under existing conditions.

Dr. Taylor's plan of management has invariably raised wages and reduced production costs simultaneously. Organizations operating under it have never once had a strike of their employees, after it has been installed long enough for them to understand its workings. Why not take the time to investigate thoroughly? The best place is right here in Philadelphia, at the Tabor Manufacturing Company and at the Link-Belt Company. A proper appreciation cannot be had by merely walking through the shops. The subject must be studied like any other scientific subject.

What better work can the Franklin Institute do than to encourage such investigation, and then to (1) announce their findings to the industrial world, (2) encourage the establishment of a government bureau for the discovery, collection, cataloging,

and dissemination of industrial methods of least waste, that, from the enormous savings thus made, the wages may be increased, hours of the working day shortened, production costs reduced, and the cost of living lessened?

MR. J. SHIRLEY EATON: The word *scientific* carries the notion of finality and precision. To attach it to any particular practice involves, as it were, an invidious distinction. Yet in a peculiar way the particular practice under discussion may lay claim to such title. It is scientific in the sense that it seeks to incorporate the latest, if not the final, idea, regardless whence it originates, so only it applies to the case in hand.

We, as railroad men, have been associated with big undertakings. The railroads were the first form of associate effort of large masses of men under corporate direction. Their methods were necessarily experimental; their early exigencies developed and brought forward men of great force of character, but that very force of character has sometimes blinded them to the methods that are the most intelligent. Business has been construed to be mere strife—almost on the plane of physical strife—and the notion of a hustling, strenuous life, which notion runs through the whole fabric of railroad organization, has resulted in pitiful wastes of men and time and nervous energy. Situations have been met as they occurred and the thing sought has been jammed through by sheer force; but it must be painfully clear to all of us that only a small part of the potentialities of men and of resources have been realized. Rules of thumb, preconceptions, and mere vague impressions have played too large a part. We have seen evidence of that in the paper here to-night, especially in the use of figures of which this "so-called" Scientific Management would not be guilty.

Scientific Management insists primarily that the unit is the basis of knowledge, and the first prerequisite of their business is to evolve and standardize these units in order that they may get the facts of experience into terms that can be used day in and day out. For three years I was on the statistical committee of the American Railway Association, trying to determine some of the fundamental units, and we did not get very far. The theme is a profound one, as it is a most imminently practical one. I do not think that we appreciate the significance, in its larger

bearings, of this movement called Scientific Management. Instead of attacking it in prejudice, we should try to comprehend the spirit of it and accelerate its achievement.

In 1835 Troy Polytechnic conferred the degree of Civil Engineer for the first time in an English-speaking country. Science and its application up to that time had been indifferently joined. It is hard to conceive that the large place achieved by the engineer of every kind in our life of to-day springs from so recent a beginning. It was my good fortune in the past few days to attend a meeting of the leaders of Scientific Management and their critics under the auspices of Tuck School of Administration, Dartmouth College. Here was sounded the latest note in the theme of the engineer and modern life. In the citadel not only of pure science, but of learning and abstract culture, the efficiency engineer made bold to enter with his instruments of precision, his tabulations, and his averages, in the application of his methods to academic life.

We may not agree with all the claims of Scientific Management, but it moves in the direction in which we are going to find the solution of our gravest difficulties of labor and capital and the way out of some of our political mazes that confuse and defeat to utter futility the public will.

In every walk of life is laid on us the responsibility to find what is efficiency, what its concrete form and what are the units in which it can be measured, so that we may accurately determine the relation of what we do to the results which we seek.

MR. B. B. MILNER: In 1904, while working in one of the larger shops of the transportation company by which I am employed, a large part of my time was devoted to the question of "Scientific Management." I became an enthusiast, having received my inspiration from a careful study of a paper upon "Shop Management" presented to the American Society of Mechanical Engineers by Fred W. Taylor, of Philadelphia.

Many practices, obviously inconsistent with the highest efficiency, came to my attention, and invariably I asked myself the question, "Why?" to which question satisfactory answers did not come for a long time. In the meantime I read and heard a great deal of the wonderful things which were being done by "Scientific Management" upon the Santa Fe, and I visited

the Topeka (Kans.) shops on several occasions, the last visit being incident to an extended trip to the Pacific Coast over the Santa Fe's lines.

A large portion of shop labor was being paid under the "Bonus System" and the number of schedules during the time the system had been in effect, together with the amount of enthusiasm with which the work was being carried on, was far in excess of that under the old *régime* of "Day Work." The improvements evidenced were those which would be expected from substitution of any reasonably well administered reward system of labor payment, free from hindrance by labor organizations, for the previous "Day Work" system.

The shop betterment organization, which introduced the "Bonus System" and also paid particular attention to tool equipment and other conditions affecting efficient operation of the shop, in an incredibly short time, introduced the use of many new, or modified, tools and methods. This betterment movement was loyally supported by Mr. J. W. Kendricks, then vice-president, and a large amount of friction between the old organization, which remained responsible for the operation of the railroad, and the corps of betterment specialists, was frequently apparent. Great as was the improvement over previously existing conditions, the shop methods and costs were apparently no more efficient or economical than those of other well-regulated shops. The methods of administration were not stable, and subsequent visits developed changes made from time to time, even in the administration of the "Bonus System" of labor payment.

Very glowing statements were made of the improvements effected, supported by numerous statements and graphical data. In some instances deductions were made therefrom which to those familiar with railroad operations could obviously not be substantiated. In some cases data submitted for the support of conclusions seemed to be at variance with actual facts. This is very naturally a result of pressure brought to bear upon "specialists" for results, and it very frequently happens that "specialists" themselves are misled, having every confidence in data which do not, upon careful analysis, support their conclusions.

It is well understood, by those familiar with railroad shop operation, that the wide variation in the cost of turning tires at various carriers' shops, referred to by Mr. Emerson, does not

reflect, in proportion to those costs, high or low efficiency for the various points from which the cost data were reported to the Interstate Commerce Commission. The labor cost of this or any other operation involves and is dependent upon (1) the volume and regularity of the work to be done, (2) the facilities, *i.e.*, tool equipment, appliances, etc., (3) the local wage scale, (4) the class of labor used upon the operation, (5) the amount of assistance given the workman or workmen, and (6) other conditions under which the operation is performed.

The comparative cost data referred to cannot mean much unless all the conditions mentioned above are taken into consideration. Failing this they become the basis of very erroneous conclusions as to relative efficiencies.

The inaccurate impressions which it was frequently found necessary to correct, and the keen reflection upon the efficiency of operation in railroad shops (none, so far as I know, excepted) culminating in the testimony presented at the recent "rate cases" heard before the Interstate Commerce Commission, have occasioned a reduction in my enthusiasm for what is popularly covered by the term "Scientific Management," and, with others in the railroad business, I have been influenced adversely by the superabundance of literature of the type mentioned specifically by Mr. Symons. The display of spectacular results said to have been obtained by the adoption of the so-called "Scientific Management System" has actually retarded the consideration of the subject, and has effected its advancement in the railroad field. Its results are so seriously questioned as to occasion Mr. Symons's paper.

With regard to the very sweeping claims which have been made relative to the saving alleged to be possible in railroad operations, two errors have been made—on the part of those responsible for these claims, the criticism most frequent and keen has been concentrated upon the operation of the carrier's shops; on the part of most shopmen, of those interested and responsible for shop operation and most of those who have followed the press on the subject, to generally include under the caption of "Scientific Management" only such details as "stop watches," "speed bosses," "premium," "tasks," "time study," "planning departments"—terms which are only applicable to certain phases of shop operation.

Most distasteful among the "Scientific Management" details

mentioned above, to outside men not in sympathy with the movement, is the "time study" idea. The strenuous objections thereto arise from the popular misconception, which should easily be relieved. The term "time study" is not specific but relative. More or less accurate "time studies" were made under the "Day Work" system, when information was gathered upon which to base an opinion as to whether this man or that had consumed more or less than a reasonable amount of time upon this or that operation. Some of these formed the basis upon which the earlier "piece work" rates were established, but their inaccuracies occasioned the largest portion of difficulties incident to operation under the "piece work" system. They may be made more and more minute, until they arrive at the point referred to by Mr. Gilbreth as the "blood-drawing type." Some "Time Study" must be the basis of every piece work, premium, bonus, contract, or reward system of labor payment, but such study only becomes objectionable as it approaches the type mentioned by Mr. Gilbreth.

Various meanings have evidently been applied to the term "Scientific Management," and it is suggested that many differences in opinion might be easily reconciled by the adoption of a common definition of the term. If "Scientific Management" means that which appeals to many upon casually reading Mr. Taylor's paper on "Shop Management," or upon first visit to a plant, such as the Tabor Manufacturing Co.'s plant in Philadelphia, where the so-called "Scientific Management" is in operation, then the term is a misnomer so far as railroad operation is concerned, and Mr. Symons must be very liberal in restricting its possible application to as many as 16 per cent. of the carriers' employees. But if the term is given the broader definition evidently attached to it by Mr. Emerson, and is understood as covering the whole field of human endeavor and as involving the elimination of every waste as soon after its development and isolation as practicable, then Mr. Symons must remove the restrictions imposed upon its application until every phase of railroad operation is covered. It then becomes applicable to the labor and material expenses, at each desk, of each office, of every department, and to all phases of operations involving expense or efficiency of service.

The economical repair of rolling equipment involves the

proper location, installation, maintenance, and efficient operation of the railroad shops, their component departments, and the tools therein, with the economical purchase of all materials required. But the economical operation of the railroad involves not only the economical maintenance of its rolling equipment (costing only about 20 per cent. of the total operating expenses), but also the efficient use of that equipment. Cars must be loaded to the maximum consistent with the service rendered, trains must also be so loaded, speeds must be those most economical, light car and locomotive mileage must be avoided, and cars and locomotives of ever-increasing efficiency, from service and maintenance stand-points, must be developed, etc. In so far as the efficiency of all these elements of operations is improved, by so much the items of labor cost, mentioned by Mr. Symons as being excluded from the application of "Scientific Management," will certainly be lowered. The carriers have specialists, inside and outside the ranks of their own employees, engaged upon the study of direct and indirect means of furthering economy in most every item of expense mentioned.

Every broad-minded railroad man has endeavored to further efficiency and knows that opportunities—"simply overwhelming"—do exist for increasing the general efficiency and economy of railroad operations. It is hoped that the following memorandum, in reply to the question of how carriers may most promptly approach and maintain maximum operating efficiency, will not be out of order. It was outlined some two years ago, after much thought, in the light of conditions existing upon one of the larger lines, and, practically without change, is offered as applicable to the operating organization of every carrier, which organization must consist of a combination of the "Divisional" and the "Departmental."

(1) The existing opportunities for further increasing the efficiency and economy of operation must be freely acknowledged, as that past creditable achievement has been obtained. Satisfaction with present operating conditions and results must be given persistent attention until an ever-increasing proportion of employees take seriously the management's desire that prompt and continued increase in operating efficiency and economy be obtained.

(2) Many identical operating problems are being solved by

independently working employees—especially the subordinates—thereby unnecessarily duplicating and wasting effort which must be conserved.

(3) The acquaintance possessed by the "staff" with details of those operations in which they are concerned must be increased in order that, through it, the general problems of which the lower officers and their subordinates should be relieved may be definitely, promptly, and otherwise satisfactorily solved—this in order that lower officers and their subordinates may be better able to cope with those problems of which they cannot be relieved.

Any deficiency in the knowledge of the higher "staff" occasions or results in: (*a*) inertia on their own part, due to a personal knowledge of its own weakness; (*b*) increased attention which their superior "line" officers must devote to the work of their subordinates, by so much taking their own time from work which they—"line" officers—cannot delegate; (*c*) weakening of the position of the higher "line" and "staff" in the minds of the lower officers, who are very quick to detect a weakness; (*d*) tabling or burying in higher offices of matters originating in the lower offices, which stultifies the ambition and constructive genius of the masses of employees, who, if properly encouraged and intelligently directed, would develop innumerable valuable ideas—"by-products" of their regular duties—with pleasure to them and at a minimum cost. From the above it follows that (1) a sharper distinction must be developed and respected between the problems regularly belonging to the higher and those belonging to the lower "staff" and "line" officers, and (2) lower officers must not expend effort in the solution of problems not properly belonging to them, without the knowledge and endorsement of higher officers concerned; (3) a knowledge and breadth of view must be developed in all employees sufficient to insure their being competent and receptive. With added knowledge and breadth of view must come an increased mutual confidence between higher and lower employees, if the number of problems confronted by either are to be actually reduced by transfer of an increasing proportion of present problems to those more expert and competent.

(4) By whatever means they may be immediately directed, all operations must be freely opened to a competent, broad-gauged inspection, conducted along such lines and in such

manner as will insure the helpful co-operation of those whose operations are inspected, and occasion the *sincere* invitation of the inspections. (The means of inspection are two, viz., (1) direct, *i.e.*, made by personal observation of the operations inspected, and (2) indirect, *i.e.*, made by the use of statements and reports which (*a*) transmit to higher officers and others, who cannot make personal inspection themselves, information gathered by personal inspectors, and (*b*) develop, through comparison of properly collated and co-ordinated records and statements of operations, or their results, operating conditions and facts pertaining thereto which could not or would not be developed by direct observation.)

In conclusion, let the term "Scientific Management" be defined and understood in the broadest sense, remove the restrictions popularly imposed upon its application, eliminate the idea that the means of its application consist so largely of the spectacular elements to which such strenuous objection is, frequently unreasonably, raised, and that efficiency is so largely evidenced by the spectacular results of which so much has been heard, and let the means of permanently instilling principles of broad "Scientific Management" into the minds of individuals composing the masses, as well as others employed by the carriers, be pushed persistently and consistently along broad lines, that results may be obtained which are permanent and real. The possibilities of such procedure cannot be accurately estimated.

GEORGE J. BURNS (communicated): Mr. Symons's paper is such a complete and comprehensive answer to the claims and pretensions of scientific managers that it is no easy matter to add to its conclusiveness. In a series of articles in *Engineering Magazine*, I am attempting to demonstrate that the most available opportunity for increased machine shop efficiency is through co-operation between the different shops to the end that each may profit by the combined experience and demonstrated advantages secured by the best.

I take decided exception to the term "scientific management" in the sense that it has come to be popularly understood, to wit: that some one, without practical experience with the work, and lacking familiarity with its requirements, is superior to a man of the same calibre, plus the experience. It must be con-

ceded that the railroads are seeking and commanding the best natural ability, developed by technical training, and are broadening it by practical experience, and profiting it by contact with men who have made the problems a life study. To sweepingly condemn such men, whose highest ambition and greatest incentive is increased efficiency, as lacking an appreciation of the principles of scientific management is an attempt to turn back the hands on the dial.

The history of railroad development refutes the charge that railroads do not welcome suggestions from the outside. Machine tool builders, co-operating with railroad mechanics, have revolutionized many, if not most, shop operations; the air-brake has made fast speed safe and heavy power practical: autogenous welding is being rapidly introduced, and is securing important economies: advanced ideas are being sought for and availed of in every department, but no one thinks of designating any one of these as scientific management. To denominate "motion study" that can be applied, as Mr. Symons has so conclusively shown, to but a small fraction of railroad employees, as scientific management of railroads, is too absurd to be seriously discussed.

## PRACTICAL EXPERIENCE AS A FACTOR IN MODERN EDUCATION

"And long experience made him sage."— GAY.

It was discovered long ago that mere theoretical instruction may stimulate the mind without controlling it. Practical training has come to be recognized as an indispensable factor. Commercial schools have their banks and counting rooms, with all the paraphernalia of actual business conditions. Law schools have their moot courts presided over by experienced judges from the bench. Normal schools give their students the experience of actual teaching. The clinic has long been recognized as an essential in the study of surgery. Manual training has become a part of grade education. Is not the school of practical railroad administration an educator and developer? Isn't a man more capable because of his service in it?

Are the exponents of motion study scientific managers? Is it possible for any one man to so far prescribe the best and fewest movements an operator shall make in performing all

classes of railroad machine shop work, as to make it safe and economical for those motions to be followed as though the operator was an automaton? I contend that it is not safe. I have before me data taken at 31 of the principal railroad machine shops in the country, showing the time and practices for preparing shoes and wedges, which, perhaps, comes nearer than any other job in a railroad machine shop to being a manufacturing proposition, and as affording an opportunity for motion study. The shortest time in any one shop is 14 minutes. Four of the shops at which data were obtained, have had the benefit and advice of efficiency engineers, all of whom are prominent in condemning the unscientific management of railroads. In one shop where the expert was permitted to prescribe the tool on which the work was to be done, he gave the time as 35 minutes. In another 22 minutes was given, but not attained. In a third shop the estimate was 42 minutes, and with the equipment and the prescribed practice, never was, and never can be, attained. In the fourth shop, the expert offered no criticism of a shop time of 40 minutes. The failure of these experts to prescribe the shortest time and the best practices is not a reflection on their ability. It is, however, an obvious commentary on their pretensions. Had these experts been given control of all shops soon enough they would, by prescribing inflexible rules, have made the shortest time impossible of attainment.

I have in mind a German professor who persuaded a Western road that he could reduce the burning of crude oil under locomotive boilers to a science. He was so well grounded in the faith that he regarded the suggestions of the foreman as impertinence. He fitted up an engine. It steamed perfectly in the yard, but when it was taken out on the road, it died on the first grade, so much to the disappointment of Herr Professor that he didn't return to the shop and explain how it happened, but left the problem to be worked out by the practical men who evolved a science from practice. In his failure the ranks of the scientific managers lost a man who would have joined them in condemning railroad management as unscientific.

In my opinion, the railroads have much to gain through a systematic study and comparison of practices in the different shops. It isn't given to any individual to see the best solution

of all problems. "Every one excels in something in which another fails" is a maxim that is older than the "Sermon on the Mount." Disraeli said: "We have put too much faith in systems and too little in men." The phases of the human mind are as diverse as the problems encountered. Every shop excels in something in which another fails. Nearly 50,000 of the best journeymen machinists in the world are studying the problems of increased railroad machine shop efficiency. Is it not safe to assume that their combined wisdom is superior to that of any theorist?

WILLIAM J. CUNNINGHAM, Assistant Professor of Transportation, Graduate School of Business Administration, Harvard University (communicated):

I thank you for the privilege of participating in the discussion of Mr. Symons's paper on "The Practical Application of Scientific Management to Railway Operation." I have read the paper with a great deal of interest and shall look forward to the privilege of reading also the discussion at the meeting and the contributed articles which will appear in the final proceedings.

For my own part, I have already gone on record on the subject in an article which appeared in the quarterly *Journal of Economics* for May, 1911, published by Harvard University, from which the following statements are taken:

The railroad man, knowing how keen is the anxiety of his profession for improvement and vigilance, has been and is proud of the achievements of American railroads. He believes that railroad efficiency is higher than the average in manufacturing establishments, and can hold its own with any line of enterprise in the United States. He thinks, too, that in the recent rate hearings the railroads should have been measured not with the exceptional industrial establishment, but with the average. He recognizes, none better than he, the existing deficiencies in railroad management; but that they are greater or more flagrant than those in other large undertakings he will not admit. The extended area of railroad activity and the problem of adequate supervision make it difficult to secure high efficiency and use of materials. The tendency of labor union policy is increasingly to trammel the manager. He is also hampered by the difficulty of securing competent men in supervisory positions.

Expert knowledge is not required to point out losses and inefficiencies. They are apparent. But criticism should be accompanied by practical remedial suggestions.

The history of American railways shows that their progress has been steady and substantial. A comparison of any two periods ten years apart will reveal impressive increases in efficiency. The net train load, for example, has increased nearly fifty per cent. in the last ten years. Such advances in nearly every case have been the result of development and improvement of existing methods and facilities. The new and improved have been the adaptation of the old; and, judging by this steady improvement in the past, it may be expected to continue in the future.

The solution of the problem of how to effect further economics and yet maintain good service seems to lie in a more rigid application of the railroad's own kind of scientific management and a continuation and enlargement of the best practices of the best railroads, so that the operating results of the least economical may approach those whose efficiency is marked, and these in turn set new and higher standards. A new system is not needed so much as a more determined and a more general application of the sound and business-like methods which have already been found effective in railroad work.

After all, there is little essential difference between the aims and accomplishments of Scientific Management as advocated by the new experts and Scientific Management as practised by the exceptionally well-managed railroads. As a system, it means a careful study and analysis of each element of operation, and the application of the methods best adapted to bring about the best results under the given conditions. Many railroads are doing this successfully; others are doing it in part. In the nature of things, however, their efforts have been directed more to the "high spots" or to those features of operation which are most in need of correction or which promise the largest or quickest returns. Scientific Management, as a system, takes a broader view and requires that the same careful study and treatment be given to every detail of operation as is given, say, to the subject of train-loading. Obviously, there is a point where this would be unprofitable—where the cost of the system would exceed the saving.

The real difference, then, between the efficiency experts and the railroads in their conception of Scientific Management is not in kind but in degree. To find a common ground means mutual concessions. On the part of the efficiency expert it will require less stress upon "system," "principles," "dependent sequences;" it will require more knowledge of the practical problems of railroads, more respect for what the railroads have accomplished, and less exaggeration and generalization concerning waste and possible savings. On the part of the railroad a more receptive attitude is needed for suggestions from the outside and a recognition of the fact that, notwithstanding commendable progress in operating economies, much yet remains to be accomplished.

Among the important features of Mr. Taylor's system of shop management, the principle of time study might well become a part of the practice of any railroad shop with a piece-work basis. The piece-work schedules of to-day are generally an evolution from "cut and try" methods. Their defects are recognized. Mr. Taylor's second principle, of standardized conditions, is equally important, and many railroad shops come reasonably close to standard practice.

But, apart from shop operation, other and greater avenues of economy are being earnestly studied. The delays and red-tape obstruction to local initiative,[1] will yield to some plan of decentralizing authority, such as is now being tried on the Harriman lines. There are undoubted economies in further standardizing of equipment and materials, as well as in improved methods of storing and distributing supplies. There is promise of economy in the experiments now being made by the American Railway Association in clearing-house accounting for joint use of cars. A substantial saving in fuel may be made by a more general adoption of the methods of the roads having the best

[1] Mentioned by W. M. Acworth, the English economist, as a defect in American railroad organization. In the same statement, made on the eve of his departure, February first, last, he expressed surprise that the newspapers should give so much space to criticism of railroad efficiency. In his opinion, American railroads are the most efficient in the world. He believes that the skeletons in the railroad cupboards have all been buried, and that now the roads "would do well to open their cupboards and let the public see how sweet and clean they are."

fuel records; and throughout the service there is crying need for more and better supervision.

Better supervision calls for better men, and to that end the educational activities of the railroads should have wider scope and more effective organization.[2] A system of management is not needed so much as managers. The system is not as important as the man. A good system will not altogether save a poor manager, nor will an imperfect system altogether hold back one who is ambitious and able. Mr. Taylor himself recognizes this in his statement, "The first object of any good system must be that of developing first-class men."[3]

Except in the important particulars of time study and functional foremanship, the system advocated by the experts and the system practised by the railroads are not very far apart. Both have for their object that which is desired by the railroads and the public—ability to give good, safe, and economical service. And if achieved, either by an improvement of present methods or by an adaptation of the new system, private management of railways will have strengthened its claim to continuance.

MR. CHARLES B. GOING, *Engineering Magazine*, New York (communicated):

If there is any part of Mr. Symons's article that merits discussion it is the statistical portion, in which there seem to be issues of fact between his statements and the figures given out officially by the road. All my statistics and data were sent away some time ago and are not immediately accessible. Offhand, I notice that Mr. Symons makes his comparisons on number of engines, irrespective of types. Types were rapidly changing on

[2] J. Shirley Eaton, in "Education in Railroad Service" (1900), says: "In the course of railroad development, there was a first era, which was the era of railroad building. Any railroad was better than a wagon road. There was next an era of co-ordination of the railroad service and finance to the commercial and financial conditions as a whole with which the railroads were called upon to deal. This was the time of the traffic organization and railroad consolidation. Next came the era of internal adjustment on the physical and mechanical side—perfection of machinery, cutting down grades, strengthening bridges, increasing the train unit. And now has come the sociological adjustment. The human part of the machine is quite as vital as the steel and wooden part in producing efficiency, and so in increasing the income."

[3] *American Magazine*, March, 1911, p. 570.

the Sante Fe during this period, much heavier engines, much more expensive to maintain, being rapidly installed. Comparison on any other basis than the tractive unit would be worthless, and where the important variable is ignored suspicion is thrown upon the whole calculation. Beyond the few figures thus open to suspicion, there seems to be nothing in the article except assertion. The officers and employees are divided into fourteen classes, which Mr. Symons takes up consecutively, apparently believing that by ascertaining of each class separately that it "would not come under the system" he has in some way strengthened his argument. Of course, it still remains a bald and wholly unsupported assertion. In fact, I am perplexed, in reading through this list, to form any idea of what Mr. Symons thinks Scientific Management is. He says that "the plan will not apply to engineers, firemen, conductors, etc., because they are all working under contracts that will undoubtedly be kept in good faith by the companies." The introduction of Scientific Management, of course, would not in the least affect existing contracts, but would offer additional reward for increased efficiency. He says: "It would not apply to section foremen, because there are only 468 more now than in 1907 and their average daily rate of pay has increased 6 cents per day, while their duties have materially increased." Under Scientific Management they would have a fairer deal. As to trackmen, he says that the plan would not apply, as the grade and character of the work will not lend itself to any such scheme. Scientific Management has been successfully applied over and over again to men of this grade and work of this character.

Of switch tenders and crossing watchmen, he says that the plan would not apply, "as there are 8,716 less in number than in 1907." What the decrease in numbers has to do with the applicability or non-applicability of Scientific Management I am utterly unable to conjecture.

Most significant of Mr. Symons's misconception of what he is talking about is the comment under Section 8, "Employees of Floating Equipment," of whom he says that "they would not be affected by this plan, as . . . their daily average wage rate of $2.31 will not admit of reduction." Mr. Symons apparently conceives of Scientific Management as some plan for cutting down wages, instead of being, as it is, a plan for making

men earn larger wages by showing them how to do better work. If further evidence is needed that Mr. Symons does not understand what Scientific Management is, it may be found conclusively in the 11th Section, "All Other Employees and Laborers." He says here, top of page 13: "The plan in question would not apply to this mixed class, as it includes the raw recruit, the man of migratory habits, and the hobo; most all are of a low order of mentality and some dissipated. *Increased supervision by practical men is the only means of increasing their efficiency or quantity of work performed.*"

This is the very genius of Scientific Management—to instruct the raw recruit, as well as every other worker, by the cooperative direction of expert practical specialists, and then, by a voluntary agreement (interfering in no way with any contract or wage agreement which may be in force), to reward him by extra incentive for any exhibition of higher efficiency in his work. The profit which employees bring to the employer through this efficiency is divided with them. It is alike within the powers of general officers, clerks, station agents, engineers, firemen, section foremen, trackmen, employees of floating equipment, telegraph operators or dispatchers, carpenters, and all other employees, to waste or to save the company's time, money, materials, good-will. Whether there are more or less of any one of these now than there were four years ago, or whether their wages have increased or decreased, has nothing to do with the commonsense proposition that by careful planning of their work (planning done by expert practical men) and by careful instruction in their work (instruction by expert practical men) they could learn to do it better, with less waste from internal or external interference, less waste from unintelligently-directed effort than exists now.

Whether the average man is firing a locomotive or tamping a tie, he can certainly learn to do it better under the guidance of the better-than-average man—the best man obtainable. If all the average men could be thus instructed, does Mr. Symons deny that it would save the company money? If this saving were shared with the men by granting them a bonus (so that there still remains on the one hand profit to the employer, and on the other incentive and reward to the employee), is not common-

sense all that is necessary to determine that sound policy and good practice have been observed? And yet this is the substance of "Scientific Management."

As Mr. Symons practically grants this proposition repeatedly, as, for example, on page 19, I am compelled to end where I began, with the simple statement that I can not discover what Mr. Symons thinks Scientific Management is, and I am therefore unable to discuss his discussion of it.

ANGUS SINCLAIR, editor *Railway and Locomotive Engineering* (communicated): It appears strange to me in this day and generation, when the production in every line of industry has been vastly increased by labor-saving machinery, that a clamor should have arisen to provide means for increasing beyond reason the output of the workman's manual skill and labor. Production of finished goods has increased so rapidly of late years that the demand of the people has not kept pace with the supply, and the whole world seems to be suffering from overproduction.

When viewed in its broadest sense, overproduction only means under-consumption; but production has increased so rapidly during the last decade that there is no time for natural adjustment. That being admitted to be the case, it is not a proper time to urge greater efforts for production upon the workman.

The assumption of the people promoting scientific management of production is that the ordinary workman is constantly trying to do as little labor as possible in return for the wages paid by the employer. I spent some ten or twelve years as a shop mechanic, and I was for about five years a shop foreman, besides spending several years visiting manufacturing establishments and railway rolling stock repair shops, for the purpose of writing about the work going on; so I have enjoyed good opportunities for judging the habits of workmen. Some men are lazy, permanently tired, and they do as little work as they possibly can, but the great mass of workmen are naturally industrious and ready to do a fair day's work. There is an individuality among workmen that enables some men to do much more work than others with the same degree of effort; but it is the kind of individuality that enables one stenographer to write

much faster than another person engaged in the same business; the want of quickness ought not to be blamed upon the person as lack of industry.

I am familiar with the work carried on in nearly all the leading railway repair shops in Europe, in this country, in Canada, and in Mexico, and I feel safe in asserting that when left to his own volition the American workman accomplishes from 10 per cent. to 25 per cent. more than any other mechanic.

Nearly all mechanics work on the principle of giving a fair day's work for a fair day's wages. In British shops there is very little inclination among those in charge to push workmen beyond their working gait, and it is generally considered rude to find fault with a man who is working steadily.

I had personal experience in the first shop I worked in in this country that gave me a striking impression of the shop etiquette here as compared with that I had been accustomed to in Scotland. I had been given a job which involved much chipping and filing, and went at it on the same speed I had been accustomed to work. When I had been working about three days, the shop superintendent stopped and watched me for a few minutes and remarked, "You know how to chip, but you're taking a hell of a time over the job. Remember, you are not in Scotland now." He was a Scot himself.

I have been watching the operation of Scientific Management, and I believe its tendency will be to take away the pride a mechanic has in doing his work—a thing to be sadly deplored. There has been so much sentiment manifested of late years to work machine tools up to their highest capacity that a similar sentiment seems to be extended to the man who is regarded as a mere animated machine, deserving no more consideration as a producer than the working parts of an automatic tool.

Another thing about the Scientific Management which I dislike is the idea of having the workman constantly under a sort of tutorage, his every movement being supervised. It seems to me that human nature will resent such humiliating conditions.

I agree with Mr. Symons that it may be practicable to introduce Scientific Management to new work, but I fail to see how repair work can be done by that system. Those supervising piece work in repair shops have great difficulty in arranging equitable pay for certain jobs, for it is often almost impossible

to estimate the time that a good mechanic will require to do the work. Repair-shop mechanics are specialists in their line. Under Scientific Management the inclination is taken away from the workman and taken up by the management. The management would need to consist of very skilful and experienced mechanics to decide in which way a difficult repair job should be executed. According to my judgment, the system would confuse the workmen without reflecting any glory on the management.

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**The Volatilization of the Electrodes in Neon Tubes.** GEORGES CLAUDE. (*Mon. Sci.*, 840, 803.)—It has been shown previously that the absorption of the luminescent gas which takes place in neon tubes is due to the volatilization or disintegration of the electrodes, and that by diminishing this by using electrodes with large surfaces the absorption becomes negligible and thus tubes can be obtained which last a long time. Moreover, if the outer skin of the metal to be volatilized has been treated with nitric acid, the gas obtained contains a very strong proportion of helium besides the neon. Three hypotheses are possible on this point: (1) A selective action of the volatilized metal on the helium contained in the neon, which is in too small a proportion to be detected by the spectroscope; (2) the transformation of neon into helium; (3) partial transformation of the neon into an ammoniacal compound by the action of the nitric acid on the neon. The experiments undertaken to solve this question have shown it was due to a selective action: a very energetic action, since it permits the whole of the helium contained in the neon (although in so small a proportion as to be invisible in the spectroscope) to be eliminated in a very concentrated condition. Sir W. Ramsay and Collie have confirmed these conclusions. Travers had already separated argon from helium by an analogous method. This selective action is due to the occlusion of helium in the copper forming the electrode.

**Action of Aqueous Solutions of Single and Mixed Electrolytes upon Iron.** J. N. FRIEND and J. H. BROWN. (*Iron and Steel Inst. Jour.*, lxxxiii, 125.)—The addition of 1 gramme of potassium chromate per litre of water, corresponding to 10 pounds of the salt per 1,000 imperial gallons of water, exerts a marked retarding effect upon corrosion. An increase in the concentration of the salt, in the presence of small quantities of sodium chloride, is even more beneficial, as 100 pounds per 1,000 imperial gallons gives excellent protection.

## NOTES FROM THE BUREAU OF STANDARDS.

### THE DETERMINATION OF TOTAL SULPHUR IN INDIA RUBBER.

**By C. E. Waters and J. B. Tuttle.**

A COMPARATIVE study was made of different methods for determining the percentage of total sulphur in a sample of rubber. The conditions were modified in many ways, the endeavor being, primarily, to effect the complete oxidation of the sulphur to sulphuric acid. It was shown that the method of Henriques may give slightly low results, probably owing to a loss of some of the free sulphur. By this method the rubber is decomposed by digestion with nitric acid; the acid is then evaporated off on the steam-bath and the residue ignited with a mixture of soda and saltpetre. The hot aqueous extract of the melt is acidified and the sulphuric acid precipitated by adding barium chloride.

In order to avoid the loss of free sulphur the authors decomposed the rubber with concentrated nitric acid saturated with bromine. Using pure, powdered sulphur, it was shown that the oxidation was complete, while with nitric acid alone a considerable part of the sulphur was not attacked, and was lost in the subsequent ignition. Blank determinations run with measured volumes of carefully standardized sulphuric acid showed that the ignition occasions no loss of sulphur trioxide.

### WAVE-LENGTHS OF NEON.[1]

**By Irwin G. Priest.**

TEN wave-lengths in the spectrum of neon have been determined by the method of flexure with reflection fringes previously described.[2]

[1] To be published in the *Bulletin of the Bureau of Standards*, vol. 8, No. 4.

[2] *Bulletin of the Bureau of Standards*, 6, 573; 1911, Reprint 142.

The following values result from a long series of determinations:

TABLE.

| | | |
|---|---|---|
| 5852.4862 | I. A. | ±0.0003 |
| 5881.8958 | | ±0.0005 |
| 5944.8344 | | ±0.0003 |
| 6074.3383 | | ±0.0004 |
| 6096.1608 | | ±0.0004 |
| 6143.0600 | | ±0.0006 |
| 6266.4948 | | ±0.0005 |
| 6304.7929 | | ±0.0005 |
| 6382.9882 | | ±0.0005 |
| 6402.2392 | | ±0.0003 |

The probable errors are in every case less than one part in 10,000,000. This indicates the *precision* of the results. Confirmatory results and counter checks indicate that the values given are *reliable*, to about one or two parts in 6,000,000. It is, therefore, expected that these results will prove accurate to within about one or two thousandths of an Angström unit A detailed discussion of the accuracy will appear in the complete paper.

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## THE HYDROLYSIS OF SODIUM OXALATE AND ITS INFLUENCE UPON THE TEST FOR NEUTRALITY.[1]

By William Blum.

IN connection with the testing of sodium oxalate for use as a primary standard in acidimetry and oxidimetry, a knowledge of its normal alkalinity is essential. Calculations based on the generally-accepted theory of hydrolysis, using the most probable values of the ionization constants, indicate that in a decimolar solution of sodium oxalate, $(H^+)$ $18° = 2.0 \times 10^{-9}$. In such a solution phenolphthalein should be converted into its pink salt to the extent of 7.8 per cent., if we accept the simple theory of indicators, and employ the value $1.7 \times 10^{-10}$ as the ionization constant of phenolphthalein. Pure sodium oxalate was prepared by recrystallization in platinum of two samples, containing respectively slight excess of acid and alkali. After two crystal-

[1] *Bulletin of the Bureau of Standards* (Reprint No. 178), and *Journal of the American Chemical Society*, 34, 123–136 (1912).

lizations, products of uniform and constant alkalinity were obtained, which was accepted as the criterion of purity. This material was found to produce a color equal to only 4 per cent. transformation of phenolphthalein instead of the calculated 7.8 per cent. Calorimetric comparisons with solutions based upon Sorensen's emf. measurements indicate that $(H^+)$ 18° for 0.1 M sodium oxalate is $2.5 \times 10^{-9}$, and that the salt is hydrolyzed at 18° to the extent of 0.0024 per cent. The error involved in a titration for neutrality, if this "normal alkalinity" is neglected, is only about 0.02 per cent., which is practically negligible in all analytical work. It was found that, contrary to statements of Sorensen, sodium oxalate solutions do not decompose on boiling, but that in glass they do become more alkaline, due to the attack of the glass. Of various kinds of glass tested, "Durax" was found to be most resistant to such solutions. It was also found that commercial samples of sodium oxalate may contain "excess $CO_2$," either as $NaHCO_3$ or as occluded $CO_2$, which is not readily expelled at 240°. Before testing the neutrality of sodium oxalate it is therefore necessary to boil the solutions in quartz or Durax glass to expel such $CO_2$.

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**Electrolytic Preparation of Bleach.** F. FOERSTER. (*Die Chem. Ind.*, xxxiv, 373.)—The theory of the production of bleach is discussed and the apparatus for its production according to Kellner, Siemens and Halske, Schuckert, Haas and Oettel, is described. Besides these, details are given of the electrolytic alkali cells of Billiter and others; the chlorine from which is afterwards absorbed by lime. These cells require less electric energy for the liberation of chlorine than the apparatus previously described. For moderate-sized textile works the direct electrolytic bleach cells appear recommendable; in other cases it is preferable to use lime chloride (bleach) or liquefied chlorine.

**Radio-activity and the Periodic System.** T. WULF. (*Anal. Soc. Sci. de Bruxelles,* xxxv, 3, 282.)—Assuming that all elements are more or less radio-active, it is to be expected that their atomic weights would differ by four or multiples of four, since this is the atomic weight of the $\alpha$ particle or helium atom. This hypothesis is supported by inspection of the table of atomic weights, for there are two series—12, 16, 20, 32, 40, 44, 48, etc., and 19, 23, 27, 31, etc.—which are the atomic weights of known elements. The atoms may therefore be considered in two groups, their atomic weights being represented by $4n$ and $4n - 1$ respectively, $n$ being any integer.

**Micrographic Examination of Metals.** J. S. G. PRIMROSE. (*Eng.*, Dec. 1, 1911, 748.)—The advantages of the use of the microscope in addition to chemical analysis and mechanical tests in the examination of metals are set forth. In the case of fractures, not only can the examination be made right up to the fracture itself, but also incorrect heat treatment or the presence of fatigue may be detected. Microphotographs are given illustrating the causes of a number of failures in iron and steel, which were revealed only by this method of analysis. These include the fracture of a wrought-iron angle bar which caused a boiler explosion; a mild steel plate which fractured on bending, although the test piece had passed the specification tests; a locomotive axle which broke while the engine was travelling at express speed; and a motor-car steering rod, the fracture of which was shown to be due to imperfect case-hardening. Attention is called to the fact that a piece of work may often be examined by the microscope before putting the material into service to determine whether it is in the proper structural condition for use.

**Compound Britannia Metal Sheet.** ANON. (*Brass World*, viii, 1, 10.)—Owing to the increase in the price of tin, Mr. Lewis B. Tebbetts, of the Hoyt Metal Company, has invented a compound Britannia metal sheet. The outside sheet is composed of Britannia metal (tin, antimony, and copper), and the base sheet is a lead alloy (lead and antimony). First the base metal is melted in a suitable open-face mold, and on this base metal, while it remains in a molten state, an unmolten body of facing metal is placed in the form of a rolled sheet. The facing body floats on the surface of the molten base metal until it becomes fused thereto. The two plates fused together are then rolled till the composite metal plate is of the desired thickness.

**"Canadium," a New Element.** A. G. FRENCH. (*Chem. News*, civ, 283.)—A supposed new metallic element, probably of the platinum group, has been discovered in the trap-dyke of the Nelson District of British Columbia. It has been found along with platinum and metals of that group in the form of grains and scales, in quantities varying from a few grains weight to three ounces per ton. This metal is white, of brilliant lustre, softer than platinum, ruthenium, palladium, or osmium, and has a melting point approaching that of silver. It is unaffected by moist air, or by heating in air for a long time. Iodine, hydrogen sulphide, or soluble sulphides do not affect it, but it is soluble in nitric and hydrochloric acids, and acts electronegatively to silver in dilute acids. It is not precipitated from its solutions by alkali chlorides or iodides.

**Pitting of Nickel Deposits.** ANON. (*Brass World*, vii, 10, 350.)—The pitting of nickel deposits may be prevented by using 5 to 6 ounces of boric acid in each gallon of nickel solution. As well as preventing pitting, it gives a whiter deposit.

# FRANKLIN INSTITUTE

*(Proceedings of the Stated Meeting, held Wednesday, February 21, 1912.)*

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 21, 1912.

VICE-PRESIDENT JAMES M. DODGE *in the Chair.*

Additions to membership since last report, 24.

The standing committees for the year 1912–13 were announced.

The following resolution submitted by the Aëronautic Section was unanimously adopted:

> WHEREAS, The Aëro Club of America proposes to establish a national aëronautical laboratory for systematic investigations in aëronautic science, and for comparative tests of aërial machines, therefore be it
>
> *Resolved*, That the Franklin Institute, through its Secretary, express to the Aëro Club of America its hearty commendation of the important enterprise, and proffer its earnest co-operation.

The paper of the evening was presented by Naval Constructor D. W. Taylor, U. S. N., on "Recent Advances in the Art of Battleship Design."

The progress made in this branch of naval architecture was described and illustrated by means of lantern slides. The protection of vessels against attack from submarines and aërial craft was considered, as well as the effect of gun-fire at various distances. The changes in design and equipment made during the past few years by the various maritime nations were pointed out.

After a vote of thanks to Mr. Taylor the meeting adjourned.

R. B. OWENS,
*Secretary.*

## COMMITTEE ON SCIENCE AND THE ARTS.

*(Abstract of Proceedings of the Stated Meeting, held Wednesday, February 7, 1912.)*

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 7, 1912.

DR. GEORGE A. HOADLEY *in the Chair.*

Mr. Wilfred Lewis was elected chairman for the year 1912.

The following reports were presented for first reading:

No. 2370—Gray's Telautograph.

No. 2511—Karn's Tunneling Machine.
Final action:
No. 2466—Tantalum Wave Detector. Action deferred.
No. 2506—Sherardizing Metallic Surfaces.
Scott Medal. Adopted.

R. B. OWENS,
*Secretary.*

## SECTIONS.

*Section of Aërial Navigation and Aëronautics.*—The inaugural meeting of the Section was held on Thursday evening, January 25, 1912, at 8 o'clock, in the Hall of the Institute. The meeting was called to order by Dr. George A. Hoadley, and after a few remarks in reference to the establishment of the Section, he introduced Dr. A. F. Zahm, of Washington, D. C., who presented a communication on "Aëronautic Laboratories, Their Establishment and Operation."

The speaker reviewed the progress made in the field of aërial navigation during the past two decades, and then described the work in the various aëronautic laboratories now in existence. The investigations made by M. Eiffel at his laboratory were described in detail.

Attention was called to the fact that the Aëro Club of America is endeavoring to establish an aëronautic laboratory in the United States, for systematic investigation in aëronautic science and for comparative tests of aërial machines.

On motion of Mr. Levy, duly seconded, it was resolved that this Section approves this effort of the Aëro Club of America, and recommends to the Institute that it offer to that club its co-operation in the establishment of such a laboratory.

After a brief discussion the thanks of the meeting were extended to Dr. Zahm.

Adjourned.

R. B. OWENS,
*Secretary.*

*Mechanical and Engineering Section.*—A meeting of the Section was held on Thursday evening, February 1, at 8 o'clock. Mr. J. A. P. Crisfield, temporary chairman.

The minutes of the previous meeting, as printed in the JOURNAL, were approved.

The Chairman then introduced Mr. Frank Shuman, of Philadelphia, who presented a communication on "Sun Power."

The problem of utilizing the sun's heat for power purposes was fully discussed and several experimental plants erected at Tacony by the speaker were described. The application of sun-power plants for irrigation and other purposes in arid regions was pointed out.

After a brief discussion the thanks of the meeting were tendered to Mr. Shuman.

Adjourned.

R. B. OWENS,
*Secretary.*

*Mechanical and Engineering Section.*—A meeting of the Section was held on Thursday evening, February 8, 1912. Mr. Louis E. Levy, acting chairman.

Prof. Lewis M. Haupt, of Philadelphia, presented a communication on "The Improvement of Our Rivers and Harbors," in which he reviewed the work which has been done in the various navigable channels and described the results obtained by the Reaction Breakwater.

The paper was illustrated by lantern slides and models.

After a brief discussion the Chairman extended the thanks of the meeting to Professor Haupt.

Adjourned.

R. B. OWENS,
*Secretary.*

*Electrical Section.*—A joint meeting of this Section and the Philadelphia Section of the American Institute of Electrical Engineers was held on Thursday, February 15, 1912. Mr. Thomas Spencer and Mr. H. Clyde Snook presided jointly.

The paper of the evening, on "Alternating Current Development in America," was presented by Mr. William Stanley, of Great Barrington, Mass. The speaker reviewed the progress of alternating current development from its beginning in this country, and described much of the early experimental work of various inventors who made it possible to bring this branch of the electrical industry to its present high state of perfection.

The subject was illustrated by numerous lantern slides.

In the discussion which followed the paper, remarks were made by Mr. Ralph W. Pope, Dr. Hoadley, Messrs. Hering, Snook, Spencer, and others.

On motion of Dr. Hoadley, seconded by Mr. Hering, the thanks of the meeting were extended to Mr. Stanley.

Adjourned.

R. B. OWENS,
*Secretary.*

## MEMBERSHIP NOTES.

### Elections to Membership.

RESIDENT.

MR. J. B. BAKER, Superintendent Philadelphia Terminal Division, Broad Street Station, Philadelphia.

PROF. JAMES BARNES, Bryn Mawr College, Bryn Mawr, Philadelphia.

MR. JOHN HAMPTON BARNES, Morris Building, Philadelphia.

MR. CALVIN W. BARWIS, Maintenance of Way Department, Broad Street Station, Philadelphia.

MR. EDWARD P. BORDEN, 2038 Spruce Street, Philadelphia.

MR. R. J. BRUNKER, The Western Saving Fund Society, Tenth and Walnut Streets, Philadelphia.

MR. EDWARD S. BUCKLEY, JR., Real Estate Trust Company, Broad and Chestnut Streets, Philadelphia.

MR. ALFRED E. BURKE, 409 Arch Street, Philadelphia.

MR. THOS. DE WITT CUYLER, 701 Arcade Building, Philadelphia.
MR. H. M. DEEMER, Secretary, The Midvale Steel Company, Box 1606, Philadelphia.
MR. BENJAMIN FRANKLIN, 1420 Chestnut Street, Philadelphia.
MR. HENRY GLOVER, 6445 Emlen Street, Germantown, Philadelphia.
MR. LINCOLN GODFREY, 248 The Bourse, Philadelphia.
PROF. ARTHUR W. GOODSPEED, University of Pennsylvania, Philadelphia.
MR. GEORGE R. HENDERSON, Baldwin Locomotive Works, Philadelphia.
MR. DAVID W. JAYNE, care Barrett Manufacturing Company, Frankford, Philadelphia.
MR. C. P. LANDRETH, 43 North Seventh Street, Philadelphia.
MR. L. R. LEMOINE, U. S. Cast Iron Pipe and Foundry Company, Morris Building, Philadelphia.
MR. SAMUEL REA, Broad Street Station, Philadelphia.
MR. A. M. SCHRAUDER, The Philadelphia Textile Machinery Company, Hancock and Somerset Streets, Philadelphia.
MR. E. R. WHITNEY, 825 North Twenty-seventh Street, Philadelphia.
MR. CHARLES WIRT, 129 Cliveden Avenue, Germantown, Philadelphia.

NON-RESIDENT.

MR. EDWARD H. BLADE, Millville, N. J.
PROF. W. S. FRANKLIN, Lehigh University, South Bethlehem, Pa.

### Changes of Address.

MR. GEORGE C. REESE, Darby, Delaware County, Pa.
MR. JAMES RICE, care E. G. Bennett, 1006 Jefferson Street, Wilmington, Del.
MR. THEODORE N. ELY, Bryn Mawr, Pa.
DR. D. S. JACOBUS, 70 Summit Avenue, Jersey City, N. J.
MR. BYRON E. ELDRED, care Commercial-Research Company, Tuckahoe, N. Y.

## NECROLOGY.

**Mr. George J. Harding**, 926 Land Title Building, Philadelphia.
**Mr. E. W. Miller**, 1222 Mt. Vernon Street, Philadelphia.
**Mr. Charles E. Ronaldson**, 4017 Locust Street, Philadelphia.

## LIBRARY NOTES.

### Purchases.

HUTCHINSON, R. W., JR.—High Efficiency Electrical Illuminants and Illumination.
LANZA, G.—Dynamics of Machinery.
HITT, R. (comp.)—Electric Railway Dictionary.
PEARY, R. E.—The North Pole. Second Edition.

KARAPETOFF, V.—Engineering Applications of Higher Mathematics, part I.
LEISER, H.—Wolfram.
ARRHENIUS, S.—Lehrbuch der Electrochemie.
HÜTTE—Des Ingenieurs Taschenbuch. 3 Volumes.
VIENNA.—Second International Congress of Refrigeration Reports and Proceedings. English edition.
WRIGHT, L.—Optical Projection. Fourth edition.
PECKHAM, S. F.—Solid Bitumens.
Massachusetts Institute of Technology, Congress of Technology, 1911.—Technology and Industrial Efficiency.
ALFORD, L. P.—Bearings and Their Lubrication.
FIELD, S.—The Principles of Electro-deposition.
BELL, J. S.—The Early Motive Power of the Baltimore and Ohio Railroad.
KARAPETOFF, V.—Experimental Electrical Engineering. 2 volumes.
LAMB, H.—Hydrodynamics. Third edition.

## Gifts.

Glasgow, Royal Philosophical Society, Proceedings, vol. 42, 1910–11. Glasgow, 1911. (From the Society.)
U. S. Navy Department. Ship's Data, U. S. Navy Vessels, 1911. Washington, D. C., 1912. (From the Department.)
Master Car Builders' Association, Proceedings of the 45th Annual Convention, held at Atlantic City, N. J., June 19, 20 and 21, 1911. Chicago, 1911. (From the Association.)
American Climatological Association, Transactions, vol. 27, 1911. Philadelphia, 1911. (From the Association.)
Princeton University, Catalogue, 1911–12. Princeton, 1911. (From the University.)
University of Cincinnati, Annual Catalogue, 1911–12. Cincinnati, 1911. (From the University.)
Yale University, General Catalogue, 1911–12. New Haven, 1911. (From the University.)
University of Nebraska, General Catalogue, 1911–12. Lincoln, 1911. (From the University.)
Georgia Geological Survey, Bulletin No. 26, Preliminary Report on the Geology of the Coastal Plain of Georgia. By Otto Veatch and Lloyd W. Stephinson. Atlanta, 1911. (From the Survey.)
Pratt Institute, Catalogue, 1911–12. Brooklyn, 1911. (From the Institute.)
United States Brewers' Association, Year-Book of the 51st Annual Convention, Chicago, 1911. New York, 1911. (From the Association.)
American Institute of Electrical Engineers, Year-Book for 1912. New York, no date. (From the Institute.)
University of Minnesota, Catalogue, 1911–12. Minneapolis, 1911. (From the University.)
Alabama Geological Survey, Bulletin No. 10, Reconnoissance Report on the Fayette Gas Field by M. J. Munn. Bulletin No. 11, Roads and Road Materials of Alabama, by William F. Prouty. Montgomery, 1911. (From the Survey.)

Iron and Steel Institute Journal, vol. 84, 1911, part 2. London, 1911. (From the Institute.)

Illinois State Mining Board. 30th Annual Coal Report of Illinois, 1911. Springfield, 1912. (From the Board.)

Massachusetts Board of Education. 75th Annual Report, 1910–11. Boston, 1912. (From the Board.)

Ministero di Agricoltura, Industria e Commercio—Statistica degli Impianti Elettrici Attivati od Ampliati in Italia nel decennio 1899–1908. Roma, 1911. (From the Minister.)

U. S. Navy Department. Navy Year-Book 1911, edited by Woodbury Pulsifer. Washington, 1911. (From the Department.)

## BOOK NOTICES.

THE EARLY MOTIVE POWER OF THE BALTIMORE AND OHIO RAILROAD. By J. Snowden Bell. 157 pages and 55 illustrations. 8vo. Cloth. Angus Sinclair Co., New York, 1911. Price, $2.

This book is a historical review of the origin and development of the motive power of the Baltimore and Ohio Railroad, the first American road of any considerable length. It includes descriptions and illustrations of the following interesting types of early locomotives: Peter Cooper's engine, the first built in the United States, operated on the road in 1830; the "Grasshopper" engines of Gillingham and Winans and the "Crabs" of Winans in 1837. Then follow Henry Campbell's locomotive with the first equalizing beams and swivelling truck; Eastwick and Harrison's engine of 1835 with single eccentric valve motion and with reversible valve seat, and the New Castle engines—the first 4-4-0 with independent cut-off. Mason's engine, put on the road in 1856, the first to be fitted with link motion valve gear and having also cylinders bolted to a cast iron saddle to which was fitted a round smoke-box, is accorded due recognition. These engines also had peculiarly arranged steam pipes connected direct to the steam-chest. Then follow Winans' "Mud-Diggers," which had horizontal cylinders and the main rods coupled to cranks on a shaft extending across the frames and geared by spur wheels to the back driving axle.

Baldwin's six and eight-wheel articulated and the "Company's" eight-wheel engine, and Ross Winans' Campbell engine, in which the cut-off was actuated by means of cams attached to the main valve, are next described. These are followed by Hayes' "ten-wheelers," which were similar to Ross Winans' engines, but had six coupled wheels and a four-wheeled truck.

Following this the author describes the Tyson ten-wheelers, built of a more modern style than the Hayes engines, having the cab on the back end, etc. He then gives an account of the controversy between the B. & O. officials and Ross Winans in regard to the most economical type of engine for the road.

A description of Perkins' ten-wheelers—built by Thatcher Perkins, who succeeded Henry Tyson—having the first Gooch link motion, precedes examples of modern practice adopted by the road.

While the book primarily relates to the history of the B. & O. Railroad as pertaining to its equipment, it nevertheless includes examples of practice without which the history of the American locomotive in general would be incomplete.

THE ELEMENTS OF GRAPHIC STATICS AND OF GENERAL GRAPHIC METHODS. By William Ledyard Cathcart, member American Society of Naval Engineers, etc., and J. Irvin Chaffee, A.M., professor of mathematics at Webbs Academy. vii + 312 pages. 159 illustrations. 8vo. Cloth. Price, $3 net. D. Van Nostrand Company, New York, 1910.

The scope and plan of this work resembles that of books of European rather than of American authorship. It embraces the theory of beams, the treatment of roof and bridge trusses, etc., and includes the graphics of friction and moment diagrams for shafting.

The fundamental theory of beams is compiled from the works of Lineham, Goodman and Cotterill. Problems are included with each section except the two on this subject. Methods are recommended for finding statical moment, moment of inertia, and the deflection of beams by using the force polygon. It is not apparent, however, how the deflection of a beam having a variable moment of inertia may be obtained by this graphical method.

On the whole, the examples in the book are well chosen and the illustrations helpful.

THE TEMPERATURE ENTROPY DIAGRAM. By Charles W. Berry, assistant professor of mechanical engineering in the Massachusetts Institute of Technology. xv + 393 pages. Illustrations. 12mo. New York, John Wiley & Sons, 1911. Price, $2.

This book, now in its third edition, is an intermediate treatise on graphical thermodynamics, and as such should be of value to technical students and others who wish to study this highly fascinating and practical application of mathematics to the theory of steam and other heat engines, etc.

The chapters are progressive, dealing with perfect gases and with working fluids, and analyzing typical cycles.

## PUBLICATIONS RECEIVED.

*Engineering as a Vocation.* By Ernest McCullough, C.E. 201 pages, 12mo. New York, David Williams Company, 1911. Price, in cloth, $1.

*Philadelphia Taxpayers Committee on City Finances.* Report for the years 1910 and 1911. 16 pages, 8vo. Philadelphia, 1912.

*The Stereophagus Pump and Special System of Pumping.* By the Hon. R. C. Parsons. 42 pages, illustrations, plates, maps, 8vo. London, no date.

*Carbonizing Coating,* the greatest preserver of iron and steel construction. 24 pages, illustrations, 8vo. Canton, Ohio, Goheen Manufacturing Company, no date.

*U. S. Bureau of Mines. Technical Paper* 6. The rate of burning of fuse as influenced by temperature and pressure. By Walter O. Snelling and Willard C. Cope. 28 pages, 8vo. Washington, Government Printing Office, 1912.

*Notice Bibliographique sur les travaux Henri Pellatt*, professeur a lo Sorbonne, professeur a la maison d'éducation de la Légion d'Honneur, president de la Société Française de Physique. Publieé par son fils Solange Pellat. 43 pages, portrait, 12mo. Paris, E. Basset & Co., no date. Price, in paper, 2 francs.

*Canada Department of Mines, Bulletin No.* 7. Western Portion of Torbrook Iron Ore Deposits, Annapolis County, Nova Scotia. By Howells Fréchette, M.Sc. 20 pages, illustrations, map, 8vo. Ottawa, Government Printing Bureau, 1912.

*U. S. Coast and Geodetic Survey.* Terrestrial magnetism. Results of magnetic observations made by the Coast and Geodetic Survey between July 1, 1910, and June 30, 1911. By R. L. Faris, inspector of magnetic work. Appendix No. 3, Report for 1911. 158 pages, quarto. Washington, Government Printing Office, 1912.

*Institute of Industrial Research*, of Washington, D. C. Bulletin No. 3, Scope and Organization. 29 pages, illustrations, 8vo. Washington, 1912.

*Metal Industry Directory*, 1912. 15 pages, 8vo. New York, Metal Industry Publishing Company, no date.

*U. S. Department of Agriculture*, Office of Public Roads, Bulletin 41—Mileage and Cost of Public Roads in the United States in 1909. By J. E. Pennybacker, Jr., and Maurice O. Eldridge. 120 pages, 8vo. Bulletin No. 42—New Hampshire Highways. Report of an inspection of highways in the state of New Hampshire, August, 1911, by Charles H. Hoyt, C.E. 35 pages, plates, 8vo. Washington, Government Printing Office, 1912.

*U. S. Bureau of the Census.* Forest Products, No. 1—Pulp-wood consumption, 1910. No. 3—Slack Cooperage Stock, 1910. No. 5—Veneers, 1910. No. 7—Wood Distillation, 1910. No. 8—Crossties purchased, 1910. No. 9—Poles purchased, 1910. 6 pamphlets, 8vo. Washington, Government Printing Office, 1911–1912.

# CURRENT TOPICS.

**The Fata Morgana.** F. A. FOREL. (*Rev. Sci.*, xlix, 24, 763.)—As is well known, this phenomenon appears in calm weather and is the apparition on the opposite bank of a lake, or of a strait, of an unexpected image of strange towns, castles, and palaces, which soon disappear. It is sometimes seen on the lake of Geneva during the afternoon of fine days in the spring or summer. M. Forel considers these appearances are due to the distribution of the temperatures in the strata of air above the lake. In the morning when the temperature of the air is lower than that of the water the horizon on the opposite bank seems to be notably lower than the true horizon, and the rotundity of the earth seems exaggerated. Towards the end of the afternoon, the air being hotter than the surface of the water, the horizon seems higher than the true horizon, and its circumference seems to recede. The Fata Morgana is seen at the moment when the one condition changes to the other.

**Starch from Potatoes.** ANON. (*Mon. Sci.* (*Mercure*), v, 840, 91.)—According to the *Chemische Industrie*, there are 500 concerns actually occupied in the production of starch from potatoes. Most of these concerns are agricultural enterprises, and only about fifty can be considered as industrial. As the yield is only 20 per cent., and the raw vegetable will not stand long transportation, these factories are chiefly found in the east of Germany. The provinces of Brandenberg, Pomerania, Posnania, Silesia, and East and West Prussia furnish one-half of the production.

**German Specifications for Roofing-felt Boards.** ANON. (*Papierfabrikant*, ix, 1440.)—With the co-operation of the *K. Preuss. Material-prufungsamt*, an agreement has been reached between the German societies representing the manufacturers and the impregnators of roofing-felt boards on the following specifications for such boards: The only raw materials permitted are rags, fibrous wastes from the textile industries, and waste paper. The selection and blending of these materials are left to the manufacturers; the addition of mechanical wood-pulp, peat, sawdust, and mineral loadings is forbidden. The percentage of ash and moisture in the air-dry board must not exceed 12. Any boards which absorb less than 120 per cent. of ordinary anthracene oil at the room temperature are to be regarded as defective. Boards of normal substance (333 grammes) and over must show a breaking strain of at least 4 kilos. on a strip 15 mm. wide cut in the machine direction. These specifications relate only to boards up to No. 150 (150 sq.m. = 50 kilos.).

**Weighted Silks.** P. SISLEY. (*Rev. Gen. Mat. Col.*, xv, 337.)—Under certain atmospheric conditions weighted silk becomes weak, and the tendering of the fibre is greatly accelerated if it is exposed to the light. It is found that ammonium thiocyanate, thiourea, and salts of hydroxylamine prevented to a large extent the tendering due to the action of light. Thus a fabric which, after exposure to light for three weeks, was weakened as much as 40 per cent., lost only 18 per cent. when previously impregnated with a 3 per cent. solution of ammonium thiocyanate or thiourea, while the same silk impregnated with a 3 per cent. solution of hydroxylamine was practically unimpaired after the same exposure. Further experiments showed that hydroxylamine and its salts are unable to prevent tendering from heat or exposure to air, while ammonium thiocyanate and thiourea largely reduced the tendering, from 64 per cent. to 10 or 11 per cent. This is explained by the fact that the tendering caused by light is due to oxidation of the fibre by stannic hydroxide, and hydroxylamine takes up the oxygen. The tendering caused by heat or air (in the absence of light) is due to traces of iron or copper acting as oxidation catalysts, and the thiocyanate or thiourea combines with iron and copper to form stable and inactive compounds.

**Ceramic Fluxes and Fusion.** R. C. PURDY. (*Trans. Amer. Ceramic Soc.*, xiii, 75.)—A flux is defined as that substance or mixture of substances which in a given mixture is the first to liquefy and become the solvent for the other components; from this starting point the idea that bases are always fluxes is combated. It is shown from practical examples that such bases as lime and magnesia often melt in a mixing after the silicates—*e.g.*, feldspar—which have been introduced as a raw material and after the eutectics which are formed, and thus act as secondary fluxes. It is impossible to classify ceramic materials into fluxes and refractories, as an excess of any component above the amount needed to form eutectics causes that component to act as a refractory.

**Electroplating Stoneware.** ANON. (*Brass World*, vii, 12, 434.)—G. Strahl, in the *Electrochem. Zeitung*, says: "The difficulty in electroplating stoneware lies in producing a durable conducting layer on the stoneware. This may be done by applying to the ware a mixture of birch-oil, asphaltum, petroleum, and 'gold-graphite,' and then burning in a muffle to 200° C. 'Gold-graphite' is made by mixing 2 grammes of pure gold chloride with 10 grammes of potassium cyanide and water and adding 1 gramme of pure graphite, and then heating to 600° C. in a muffle. A mixture containing 12 parts of 'gold-graphite' to 5 parts of petroleum is recommended. The ware should be finally burned in a strongly-reducing atmosphere to form a carbon deposit in the outer layers of the pottery; this is said to increase the tenacity of the metallic deposit."

**Effect of Temperature on Dielectric Strength of Porcelain Insulators.** C. E. HENDERSON AND G. O. WEIMER. (*Trans. Amer. Ceram. Soc.*, xiii, 469.)—A porcelain insulator of the composition 27 per cent. of Tennessee ball clay, No. 7, 6 per cent. of Tennessee ball clay, No. 3, 16 per cent. of Georgia kaolin, 16 per cent. of American eureka flint, and 35 per cent. American eureka feldspar, was found to become a conductor at or about 400° F.

**The Origin of White Residual Kaolins.** H. RIESS. (*Trans. Amer. Ceram. Soc.*, xiii, 51.)—Six theories of the origin of white residual kaolins are reviewed, both historically and with regard to the arguments pro and con. Five agree in ascribing the parentage of kaolin to feldspar, and they explain its formation by: (*a*) Simple mechanical weathering, (*b*) post-volcanic emanations (sometimes called the pneumatolytic theory), (*c*) ascending spring waters containing carbon dioxide, (*d*) waters draining from swamps or peat bogs, charged with humic and carbonic acids, (*e*) sulphuric acid or sulphate solutions. The sixth and last theory regards kaolin as the decomposition product of muscovite. The author considers that all the workable deposits of the United States and many in Central Europe have been produced by the action of surface waters, whether entering direct from the surface or filtering through a swamp or bed of peat.

**Adsorption of Radio-active Substances by Colloids.** E. EBLER AND M. FELLNER. (*Zeit. Anorg. Chem.*, lxxiii, 1.)—It is probable that the prevalence of radio-active materials in mineral springs and their sediments is partly due to adsorption by the inorganic colloids which are always present. Radium salts are readily adsorbed by the colloids of rubber, so much so that special precautions are necessary when radium bromide is kept in an ebonite capsule. Radio-lead (radium D, E, and F) can be freed from inactive lead and made richer in radium F (polonium) by fractional adsorption with gelatinous silicic acid. Radium emanation is readily adsorbed by silicic acid gel, and is more soluble in silicic acid sols than in distilled water. Mixtures of uranium and uranium X can be separated by repeated adsorption with silicic acid gel, as the uranium X is much more readily adsorbed.

**Copper Tubes for Automobiles.** ANON. (*Brass World*, vii, 12, 419.)—Copper tubing is much more reliable than brass for automobile work, whether for conveying gasolene, acetylene gas, compressed air, or oil. Brass is apt to crystallize on account of the constant vibration and the bending of the tube back and forth, and the breakage of one of these pipes simply cripples the car. Copper does not crystallize readily, and may be considered "safe," under such strains of vibration, shock, and bending as would assuredly crystallize brass.

**Solubility of Radium Emanation.** R. W. BOYLE. (*Phil. Mag.*, xxii, 840.)—This article describes experiments on the solubility of radium emanation in water and other liquids, which show that Henry's law is valid; at any one temperature the coefficient of solubility (*i.e.*, the ratio of the concentration of the emanation in the liquid to its concentration in the gas acting as a carrier) is independent of the concentration of the emanation, and of the nature and pressure of the other gas, and depends only on the nature of the absorbing liquid. In comparison with many ordinary gases the emanation is fairly soluble in water, as the following figures show, at the chosen temperature of 14° C. Coefficients of solubility: hydrogen, 0.02; nitrogen, 0.02; oxygen, 0.04; ethylene, 0.17; nitric oxide, 0.28; nitrous oxide, 0.81; carbon dioxide, 1.1; hydrogen sulphide, 3.3; radium emanation, 0.303. As to variation by temperature, the emanation coefficient at 0° C. is 0.507 and at 40° C. is 0.153. At 14° C. a quantity of emanation will distribute itself between equal volumes of water and gas so that about one-quarter will go to the water and three-quarters to the gas; at 0° C, one-third will go to the water and two-thirds to the gas. Mercury does not absorb the emanation; sea-water of specific gravity 1.022 at 14° C. absorbs about 0.84 times as strongly as water. Organic liquids absorb very strongly. At 14° C. ethyl alcohol (absolute) absorbs 24 times, amyl alcohol 31 times, and toluene 45 times as strongly as water.

**Chemical Porcelain.** G. A. MURRAY. (*Trans. Amer. Ceram. Soc.*, xiii, 585.)—Chemical porcelain is designed to meet sudden changes of temperature. The best mixtures for this purpose contain 15 per cent. or less of feldspar and about 40 per cent. of clay. If the proportion of spar be higher, then a high content of clay or flint is better than a nearly equal quantity of each. Bodies of relatively high absorption withstand cracking better than those of low absorption, except in the case of bodies high in flint.

**Pitch of the Sound Produced by Alloys and its Variation with the Temperature.** F. ROBIN. (*Comptes Rendus*, cliii, 665.)—In the case of tuning-forks of alloy steels, it is found that chromium raises the pitch of the sound produced, while nickel lowers it, and that the pitch is at its lowest in an alloy containing 36 per cent. of nickel. The influence of variation of temperature on the pitch of the sound is discussed, and it is shown in the case of nickel-steels that this effect varies considerably according to the nickel content. The following alloy steels (containing about 0.1 per cent. of carbon) are recommended for the production of tuning-forks which will be practically unaffected by change of temperature within the limits given: 44.5 per cent. of nickel and 2 of manganese unaffected between 0° to 50° C.; 47 per cent. of nickel and 3 of chromium, between 0° and 60° C.; 42.4 per cent. of nickel and 1.5 of chromium, between 0° and 100° C.; and 45 per cent. of nickel, between 0° and 100° C.

**Sand for Extinguishing Fires.** IRVINE KEMPT. (*Amer. Mach.*, xxxv, 25, 1159.)—Mr. Kampt has failed to notice any reference to the efficiency of sand as an extinguisher in the notices of fires in factories. His experience is that in factories in which alcohol, kerosene, and gasolene were in daily use buckets of sand were kept in easy reach and repeatedly proved an effective means of smothering fire before it became serious. Of course, sand is not desirable among machinery, but that is preferable to total destruction. Factory fires will not be materially decreased till there is more intelligent inspection upon the part of the fire underwriters. The employment of men who know something of factory life and methods, and with the necessary courtesy and firmness to secure the carrying out of their recommendations for the prevention of fire, will be a long step forward. (The abstractor can verify the value of sand as a fire-extinguisher from personal experience in oil factories.)

**Protection from Rust.** ANON. (*Brass World*, vii, 12, 432.)—Iron and steel can be satisfactorily protected from rust by electrogalvanizing. This does not compete with hot galvanizing to any great extent, but has replaced coverings of other metals, such as nickel or copper. As a protection against rust, electrogalvanizing has been found far superior to these metals.

**Wood Distillation in Sweden.** ANON. (*Oil, Paint and Drug Rep.*, Dec. 4, 1911.)—The chief products obtained by the destructive distillation of wood are charcoal, wood naphtha of 64°, methyl alcohol, pyroligneous (crude acetic) acid, technical and crystallized acetic acid, formaldehyde, creosote, turpentine, lampblack, tar oil, pitch, etc. Little effort is made to further isolate the other constituents, or to use those separated for the manufacture of other derivatives. Forty years ago factories were established in Sweden for the destructive distillation of the stumps of spruce and other conifers, to obtain the crude wood oil, used as a substitute for turpentine. This manufacture has not been very successful, as the product can not be obtained free from a pronounced burnt odor. Six small factories are engaged in this distillation. For a country so rich in conifers as Sweden the production of resin and turpentine is very limited. Four hundred tons of turpentine are imported annually.

**Source of Vanadium.** ANON. (*Brass World*, vii, 12, 420.)—All the vanadium now used in the United States is practically obtained from one source. Extensive deposits of vanadium ore are located in Peru, South America, and these are controlled by American interests.

**Manufacture of Rubber Molds.** ANON. (*Brass World*, vii, 12, 426.)—Metals which are not acted upon by sulphur should be used, therefore the copper or lead alloys are not serviceable. Aluminum, iron, or tin alloys, such as Babbitt metal, are useful for this purpose.

**Degumming Silks by the Froth of Soap Solutions.** G. COLOMBO AND G. BARONI. (*Rev. Gen. Mat. Col.*, xv, 342.)—Schmid Frères patented a machine for degumming silk by hanging it in the froth from a boiling soap solution. This process, which is extensively used in the industry, is compared with the ordinary process of boiling the silk in a soap solution. The conclusions reached are: (*a*) The froth method extracts the sericin from raw silk as completely as the immersion process, but the silk loses less weight, because a small quantity of soap remains on the material. (*b*) The process is completed more quickly with the new than with the old method. (*c*) The influence of hard water is no greater with the "froth" process than with the ordinary process. (*d*) If the silk has been treated with soaps, fatty substances, or other agents to facilitate the working of the fibre and subsequently conditioned, the froth method, like the ordinary soaping process, is not so efficient. (*e*) The strength and elasticity of the fibre and its behavior towards weighting substances or dyestuff solutions are the same after both processes. (*f*) The froth process leaves the fibre in a better condition as regards handle and lustre. Moreover, as the fibres are less agitated during the process, they are in a better condition for subsequent working.

**The Sun.** ERNEST COUSTEL. (*Revue Sci.*, xlix, 24, 745.)—An exhaustive article touching on the importance of solar study, the constitution of the sun, the solar dusts, and the solar heat. At the conclusion Svante Arrhénius's theory of the constitution of the sun is given, which may be summarized as follows: The sun is a gaseous mass, strongly compressed and in constant agitation. The temperature of its outer surface, where the condensations occur, is about 6,000 to 7,000° C., but this increases from the periphery to the centre at about 9° per kilometre, so that the centre has a temperature of 6 million degrees. Hence the bodies which exist in the deep beds are compounds which have absorbed enormous quantities of heat in their formation. Arrhénius writes: "These quantities compare with those which analogous combinations absorb on our globe approximately as the temperature of the sun is to that of the earth." When these compounds are forced to the surface by the internal movements revealed to us by sun-spots and protuberances, they decompose, disengaging the heat which they absorbed during formation. "They may be considered," says Arrhénius, "as explosives of enormous force, in comparison to which dynamite and the picrates are mere toys." This may explain the inconceivable speed which the protuberances attain, such as the enormous protuberance observed by Borelli at Guelma during the total eclipse of 1905, which covered considerable space in an instant. If the sun were merely a globe of incandescent carbon, burning in the same way as the combustibles which feed our hearths, its reserves of heat would be dissipated in 6,000 years. But the energy it contains due to these endothermic

combinations are sufficient to supply heat for *four thousand millions* of years; and if we add to that the help of the meteorites, the effect of the progressive condensation under the influence of attraction, as well as the probable, though not proven, rôle of radio-active matter, it is easy to conceive that the sun has been able to maintain the heat which it radiates around it from a time far anterior to the most distant geological epochs. We can also understand that its temperature has not sensibly decreased during the ephemeral duration of our historical period. These considerations are no less reassuring for the future. The hearth upon which the existence of our planet, or at least of its inhabitants, depends will not appreciably cool for thousands of millions of years, perhaps even not for thousands of millions of centuries. Nevertheless, if remote, we can imagine that the final catastrophe will at last arrive. A time will come when the star which lights us will only emit the infra-red radiations, but the human race will have disappeared long before that. Then the solar system, having become invisible, will continue to wander in space, until the instant when meeting an analogous system the shock of the two extinct stars will prepare a new genesis.

**The Calibre of Naval Guns.** ANON. (*Rev. Sci.*, xlix, 24, 760.)—It appears that in all navies there is a tendency at the present time to increase the calibre of the guns. In fact, Great Britain has adopted 13.5 inches, which equals 343 mm.; the United States, 14 inches, or 355 mm., and Japan, the same calibre for its new guns. In Germany, it is said, a calibre of 352 mm. will shortly be adopted. The object, naturally, is to increase the destructive power, which increases with the increase of the explosive charges it is possible to place in projectiles of a larger diameter. It must also be considered that heavier projectiles do not require so high a muzzle velocity, and yet have just as much force on reaching the target. The decrease in velocity lessens the erosion of the bore of the gun; and this erosion is greatly dreaded just at present. Vice-Admiral Mason, of the United States, estimates that the life of a gun does not exceed 100 to 150 rounds on account of this erosion. At the same time he states that it is scarcely necessary to increase the range of the gun more than 9 kilometres (about 5½ miles), because at that distance nearly all of the armored portion is below the horizon, even to those who sight the gun at a height of 7.5 m. (about 24½ feet) above the sea-level. At a range of 13.5 kilometres (about 8 miles), which is sometimes thought desirable, the whole hull of the ship is below the horizon, and all that can be seen is the superstructure, at which it is very difficult to aim.

**Preservation of Wood.** HÖNTSCH UND CO. (*Ger. Patent* 239,697, 1910.)—Acetone oil alone may be used instead of a mixture of acetone oil and rosin for impregnating wood, especially if the wood is not subsequently to be exposed to the direct action of the sun.

**General Rules for Annealing Rolled and Forged Carbon Steel.** H. M. HOWE ET AL. (*Eng. News,* lxvi, 188.)—These rules were drawn up by the Committee of the American Society for Testing Materials on "Heat Treatment of Iron and Steel:" (1) Purpose.—Annealing removes coarseness of grain and serious internal stresses due to rolling or working the metal. (2) Method of Heating.—The temperature should be held long enough at the annealing point to insure complete heating. (3) Control of Temperature.—Use of trustworthy pyrometers. (4) Control without Pyrometer.—Strong lights surrounding the furnace should be avoided. Allowance must be made for brighter surroundings by day than by night. (5) Magnetic Indications.—With steel of 0.5 to 0.9 per cent. carbon, the metal suddenly ceases to be magnetic at the annealing temperature. (6) Annealing Temperature.—In general, the higher the carbon, the lower the annealing temperature. (7) Annealing Ranges are Given.—Carbon less than 0.12 per cent., 875° to 925°; carbon 0.12 to 0.29 per cent., 840° to 870°; carbon 0.30 to 0.49 per cent., 815° to 840°; carbon 0.50 to 1.00 per cent., 790° to 815°. (8) Care in heating.—The flames should never touch any part of the object heated. (9) Cooling.—The higher the carbon, the slower must be the cooling. The slower the cooling, the softer and more ductile the metal will be, and the lower its tensile strength, elastic limit, and yield point. (12) To give an unusually high ductility with tensile strength and elastic limit, quench in water or oil. Anneal within a few hours after quenching, maintaining a temperature of at least 100° during the interval. For high elastic limit and tensile strength anneal at 500°; for intermediate elastic limit and tensile strength, at 600°; and for greatest ductility at 725° to 750°.

**Movements of Solar Prominences.** H. DESLANDRES. (*Comptes Rendus,* cliii, 221.)—There appears to be evidence of the existence of strong ionization and a magnetic field in the region of the upper solar atmosphere. Most of the high prominences are found to contain the gases hydrogen, helium, and also calcium. If the rotation and deviation of the prominence matter is really due to the magnetic field acting on the ionized particles of these gases, the effect should vary with the atomic weight of the substance. Visual observations and photographic records of prominence changes also afford evidence that the phenomena may be the result of magnetic rotative action. Systematic registration of the radial motions of prominences is suggested as an important research which would give valuable data for the theoretical discussion in detail on these lines.

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